

# CRYPTOZOLOGY

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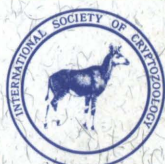
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## WHY CRYPTOZOOLOGY?

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**ABSTRACT:** One of cryptozoology's main concerns is the possible survival of animals thought to be long extinct. Because the geological record is incomplete, the possibility of fossil animals surviving to the present cannot be ruled out. Comparison is made between the objects of study of zoology and the non-animal subjects of other disciplines, including botany, and the conclusion is drawn that the elusiveness of some animals is the main reason for the existence of, and need for, cryptozoology. Given the nature of cryptic animals, cryptozoology has to apply its own methods of research.

## INTRODUCTION

Since the article "What Is Cryptozoology?" by Bernard Heuvelmans (1982), published in the first issue of this journal, has not made critics any less vociferous (May 1984, Simpson 1984, Diamond 1985), it seems worthwhile to devote more attention to the *raison d'être* of the discipline. One of cryptozoology's main concerns is the possible unrecognized "persistence of presumed extinct animals to the present time or to the recent past" (*The ISC Newsletter* 1982). Consequently, we should examine the problem of extinction and survival in relation to cryptozoology.

## THE EXTINCTION-SURVIVAL DICHOTOMY

Recent attention to the notion of species extinction has somewhat obscured the phenomenon of species survival. Both extinction and survival are *conditio sine qua non* of biological evolution. But, man's intervention apart, dying and living in nature are so propitiously balanced that the variety of life and the number of species on earth have generally been on the increase for millions of years, this being due not only to the ongoing process of speciation, but also to the persistence of old forms.

This being so, how do we know precisely what is extinct and what is extant in biology? According to Charles Darwin (1929: 280, 299), "No fixed law seems to determine the length of time during which any single species or

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any single genus endures,” and “the utter extinction of a whole group of species has sometimes been a slow process, from the survival of a few descendants, lingering in protected and isolated situations.” A noted Soviet paleontologist, L. S. Davitashvili (1948: 486), has this to say on the subject: “It is always necessary to remember the incompleteness of the geological record. The first appearance of a given species in the geological record and its disappearance from the latter can in no way be taken for the dates of its origin and final extinction. The real life span of a species (or a group of species) is usually much longer than the period determined from the geological record. Consequently, the dating of the extinction of a form or a group is not as simple a matter as may appear from the frequent citing in the paleontological literature of extinction dates for various organisms.” (Author’s translation.)

Put another way, it is a matter of observation, of empirical investigation, whether an organism is extinct or extant. And here there is an interesting difference between specimens and species. If a skeleton is found, it means a specimen is dead for good; and as long as a species (or other taxon) is known *only* from the fossil record, it is presumed to be extinct. But nothing prevents its “rising from the dead” the moment its living representatives are discovered. This having happened a number of times, there is nothing highly unusual or unscientific in expecting or predicting further occurrences of this kind.

What irritates critics is that some cryptozoologists are willing to acknowledge the possible survival today of dinosaurs and pre-*sapiens* hominids. Let us note, in this connection, that the wholesale dying out of the dinosaurs had intrigued paleobiologists long before the advent of modern cryptozoology, and that, despite many attempts, no fully satisfactory explanation of the phenomenon has yet been offered. Indeed, who can convincingly explain why such reptile forms as crocodiles, lizards, snakes, and turtles survive today, but dinosaurs—which, incidentally, were of different sizes and had the widest range of distribution—do not. Or, indeed, why gibbons, orangutans, chimpanzees, and gorillas are still living, but not *Gigantopithecus*, *Australopithecus*, *Homo erectus*, or *Homo sapiens neanderthalensis*? Survival of any of these forms thought to be extinct would actually solve, not pose, a problem. For we do know that they existed, and we can only speculate why they ceased to exist.

Thus, cryptozoology not only follows established scientific principles, but attempts to answer some topical questions confronting zoology today.

#### HIDDEN OR HIDING?

If the subjects of cryptozoology are “hidden” animals, then many sciences have their “hidden” objects: astronomers attempt to detect unknown celestial bodies “hidden” in the vastness of space (some of which cannot even

be observed optically); geologists search for minerals “hidden” in the earth; paleontologists search for “hidden” fossils, etc. Why, then, don’t we have cryptoastronomy, cryptogeology, etc.?

Up to a point, the history of exploration in botany is remarkably similar to that in zoology. The age of discovery in both is far from over. In the eighteenth century, Linnaeus described some 7,000 species of higher plants. Today, botanists know over 350,000 species of higher plants, and, according to specialists at Moscow University, not less than 2,000 new species of lower and higher plants are described every year from all over the world. The world flora, just as the world fauna, is characterized by the coexistence of evolutionary old and new forms. “Living fossils” are known among plants as well, as, for example, *Metasequoia glyptostroboides*, first described by paleobotanists and later found alive. And botanists also often depend on the testimony of native peoples in botanical exploration and the discovery of new species. Yet, there is no cryptobotany, nor is there going to be. Why not?

The answer is too obvious to be always noticed. There can be spectacular plants “hidden” in the jungle (e.g., *Victoria regia*), but there are no plants *hiding* anywhere. The difference between zoology and all other disciplines engaged in the study of inanimate or non-animal objects, is that the subjects of zoology have the *ability* to hide, and thus remain unknown or only vaguely known to science.

Two diverging behavioral strategies seem to have evolved to enhance the survival of species. Many species are highly open and visible—some even moving in herds and flocks—and usually have naturally high population densities; these species are generally well known to man. Other species, however, are less visible, and their population densities may be low; they survive because their secretive ways are adaptive.

For example, crows, pigeons, and sparrows are commonly observed by all, but it is unusual to observe an owl in the wild. Even more interesting are examples of these differing behavioral adaptations among related species: crow-jay; giraffe-okapi; baboon-orang-utan.

Of course, we know little or nothing about the population densities or reproductive rates of so-called “cryptids” (i.e., the subjects of cryptozoology), but there seems to be little doubt that the larger ones must be among the rarest of living species. It is reasonable to suppose that giant octopuses are not as numerous as those of moderate size, that sauropods, if extant, are not as numerous as turtles, and that Sasquatches (whatever form of pre-*sapiens* hominid or hominoid the species may represent) are not as numerous as bears in any given area.

It is difficult to find a rare mineral, it is time-consuming to locate a rare plant, *but it is many times more difficult and time-consuming to discover a cryptid, not only because it is rare, but also because, in contrast with minerals*

and plants, it attempts to avoid detection. To borrow Darwin's phrase, cryptids are "lingering in protected and isolated situations," such as deep waters, thick vegetation, or mountainous terrain. Being active and mobile, cryptids inevitably leave, from time to time, the depths of their sanctuaries and come into view of human beings. But they invariably retreat to their natural strongholds as soon as humans come in force to "discover" them. Hence, a cryptozoological situation: an animal is occasionally sighted by a local people or by visiting outsiders, but is absent from zoology's inventories of the Animal Kingdom.

That is the main answer to the question "why cryptozoology?" But there are also auxiliary ones, such as the difficulty of photographing elusive animals in the wild, the difficulty of retrieving specimens from remote areas, and the propensity of native peoples to attach certain beliefs to rare animals.

#### METHOD IN MADNESS

*Webster's Collegiate Dictionary* gives the following definition of science: "Science is systematized knowledge considered in reference to the discovery or understanding of truth."

First, it should be noted that the systematizing of knowledge and the discovery of truth are supposed to take time. Secondly, each discipline uses its own methods of discovering truth, methods that are best suited for its own particular purposes.

Let us spell out some common terms in cryptozoological research: eyewitness; testimony; identification; tracks; footprints; surveillance; reference files; photographs; laboratory analysis. Has all this been consciously borrowed from a criminal investigations department? Not at all. The remarkable coincidence of terms came about quite naturally and spontaneously because criminologists and cryptozoologists utilize similar methods.

The first order of business in cryptozoology is to collect and analyze the testimony of witnesses. There is no end to mirth, scoffing, and solemn warnings from critics in this connection. When preparing for an expedition in search of the Yeti, Charles Stonor (1955: 7) was given this advice by a zoologist: "Do remember that a Native [sic] will always tell you what he thinks will please you and what you want him to say." And here is a warning from none other than George Gaylord Simpson (1984: 12): "Humans are the most inventive, deceptive, and gullible of all animals."

It is noteworthy that such an attitude is generally evinced by individuals whose professions are not concerned with collecting and analyzing human testimony. Of course, all testimony is subject to doubt, but it is one thing to state that men are fallible—or that some people are fond of spinning a tale—and quite another to assert that no man is trustworthy, and no witness can tell the truth.

The role of the witness is often overlooked because it is taken for granted.

To show that this kind of evidence is valid not only in court, I shall refer to the physicist's knowledge of a phenomenon called ball lightning. Since there is no generally accepted physical theory for this phenomenon, the reality of ball lightning is doubted by some physicists, who suggest that it is an illusion caused by the effect of a flash of ordinary lightning on the retina of the eye. To determine the truth, a team of Soviet physicists, headed by I. Imyaninov, has collected about 1,500 eyewitness reports of ball lightning worldwide. These reports, both published and solicited, were subjected to computer analysis. The Soviet government newspaper *Izvestia* reported on September 30, 1976, what Imyaninov stated about their findings: "Collective portraits of the phenomenon made by eyewitnesses of different nations and at different times have coincided, and this in itself is telling proof of the reality of ball lightning." Similarly, in cryptozoology the process of "demythification" of evidence starts with the analysis of eyewitness accounts.

The next most important—and for terrestrial cryptids, inevitable—kind of evidence is tracks. This would seem to be perfect zoological material, but many zoologists simply do not accept tracks of unknown animals. Fortunately, on this subject cryptozoology finds support in paleontology, which is concerned, among other things, with the footprints of unknown, albeit extinct, animals. For example, Mossman and Sarjeant (1983: 75) state the following: "The skeletons of extinct land animals in museums suggest that the main fossil evidence for such animals is bones. This is not the case. The bones of the animals are very much rarer than their tracks. Indeed, many extinct land animals are known only from their tracks; their bodily remains appear never to have been preserved." The authors even find certain advantages of this kind of evidence compared with bones: "The animal's tracks and trails . . . are a much more extended and dynamic testament. Studying these clues to an animal's behavior, the paleontologist is able, as it were, to see the animal in action."

In cryptozoology, footprint evidence plays a decisive role in probing the reality and identity of the so-called relict hominoids (Yeti, Sasquatch, etc.). The best studied is the collection of footprints of the North American Sasquatch (Green 1978, Hunter with Dahinden 1973), which occasioned Krantz (1972: 103) to state: "No matter how incredible it may seem that the sasquatch [sic] exists and has remained uncaught, it is even more incredible to believe in all the attributes of the hypothetical human track-maker . . . . Even if none of the hundreds of sightings had ever occurred, we would still be forced to conclude that a giant bipedal primate does indeed inhabit the forests of the Pacific Northwest." Demythification of the Sasquatch footprint evidence has been further advanced in subsequent works (Krantz 1983, Bayanov, Bourtsev, and Dahinden 1984a, Buckley 1984).

There is no denying that such footprints can be, and are known to have been, faked. However, the crucial question is not whether some tracks have



been faked, but whether some are authentic. The latter question is answered by the investigators involved with a most emphatic "yes." And, to cite Napier (1973: 203), "if *one* track and *one* report is true-bill, then myth must be chucked out of the window and reality admitted through the front door."

A major "tool" of cryptozoology is photography, both still and motion picture. Again, the critics maintain that photographs and films are easily faked, and therefore cannot be accepted as evidence. And, again, these opinions come from individuals who do not vitally depend on photography in their professions.

A lot of work has been done on, and much information obtained from, the Patterson-Gimlin Sasquatch film, the study of which I was fortunate to participate in. We have subjected the film to a systematic and many-sided analysis. We have matched the evidence of the film against the other categories of evidence, and tested its subject with our criteria of distinctiveness, consistency, and naturalness. Our conclusion: "The Patterson-Gimlin movie is an authentic documentary of a genuine female hominoid, popularly known as Sasquatch or Bigfoot, filmed in the Bluff Creek area of Northern California not later than October 1967" (Bayanov, Bourtsev, and Dahinden 1984b: 232).

Let me stress that, given the nature of cryptids, and barring their chance discovery, their *planned* discovery, which is the hoped-for intent of cryptozoology, is almost certain to be preceded by eyewitness, footprint, and photographic evidence. Thus, the existence of such evidence, and the study and evaluation of it, is a clear indication that cryptozoology is proceeding appropriately as a particular line of scientific inquiry.

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## FOSSIL UNGULATE MAMMALS DEPICTED ON ARCHAEOLOGICAL ARTIFACTS

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**ABSTRACT:** New evidence is presented of archaeological artifacts that may depict fossil ungulate species that survived into relatively recent (historical) times. Following Colbert's (1936) report of the depiction of a probable sivathere on a Sumerian chariot ring, further evidence indicates possible depictions of the sivathere genus *Bramatherium* on a variety of Eurasian artifacts. Other possible depictions of extinct ungulates include a chalicothere from the Siberian frozen tombs (500-400 B.C.), a giant aquatic hyrax (*Pliohipyrax*) from a sculpture from the late Chou Dynasty of China (600-500 B.C.), and a Pleistocene antelope (*Mesimbroportax*) from a recent African Tji Wara sculpture.

### INTRODUCTION

The term "ungulate" means "bearing hooves"; that is, with the terminal digit on the limbs being covered by a horny structure composed of keratin, that is used for bearing the animal's weight. The possession of hooves on the feet is a derived condition for animals, and was evolved from the type of keratinous claw that characterizes the terminal digit of most land vertebrates. (The flat nail possessed by primates is another modification of this primitive type of claw, although here the modification is obviously not as extensive as in the development of a hoof.)

The term "ungulate" is habitually used to refer to hoof-bearing mammals, such as horses and cows. Although mammals are the only living vertebrates that bear hooves, this was not always the case in the past. Hoof-like structures were also characteristic of dinosaurs in the order Ornithischia, such as the horned *Triceratops*, and were also seen in certain other extinct reptiles, such as the early Tertiary terrestrial sebosuchian crocodiles. It seems that hooves are evolved in certain large animals (i.e., over 5 kg in body weight), that have adapted their limbs for relatively fast locomotion, and that stand with a limb position equivalent to that of a ballet dancer standing on point (the technical term for this is "unguligrade"), so that the terminal digit is the only part of the foot that bears the animal's weight. Most, but not all, hooved vertebrates were herbivorous.

Present-day ungulate mammals belong to the orders Artiodactyla (pigs and related animals such as hippos, cattle, antelope, deer, and giraffe) and Perissodactyla (horses, rhinos, and tapirs). Some other living mammal groups are usually termed "subungulates," presumably because their hooves are not as well defined as the true ungulates. These comprise the related orders

Proboscidea (elephants and their fossil relatives, such as mastodonts), Hyracoidea (hyraxes, small rabbit-like animals which are probably best known as the "conies" mentioned in the Bible), and Sirenia (sea cows, which have obviously secondarily lost their hooves in the process of becoming aquatic). Hyraxes resemble large rodents, but are in actual fact small ungulates with nail-like hooves, four on the front foot, and three on the back foot. They have what is termed a "digitigrade" foot stance (i.e., standing on the human equivalent of tiptoe, like a dog or a cat), rather than the unguligrade stance of other living ungulates.

Extinct ungulate orders include the South American Notoungulata and Litopterna, and the rather poorly defined early Tertiary Condylarthra, which is assumed to be the "basal stock" for all other ungulate and subungulate orders—although this issue is still in dispute, and the "condylarths" probably do not represent a distinct unified lineage (Prothero, Manning, and Fischer 1988). All hooved mammals (with the exclusion of the extinct order Pantodonta) have recently been shown to be a "monophyletic clade" (i.e., to all have a common ancestor). Indeed, the various "subungulate" orders are more closely related to perissodactyl ungulates than to artiodactyls (Prothero, Manning, and Fischer 1988). Thus, it is now appropriate to use the term "ungulate" to refer to almost all hooved mammals.

The best known description of fossil ungulate that may have survived into historical times, as documented by its apparent depiction in the archaeological record, is that by Edwin Colbert (1936) of a sivathere (a type of giraffe), depicted on an artifact from Iraq dated at 3500 B.C. A published account also exists of a possible example of "Irish elks" (genus *Megaloceros*) depicted in artifacts from the Black Sea region dated at around 600-500 B.C. (Bachofen-Echt 1937), although the last fossil evidence of this animal is from the end of the Pleistocene, about 10,000 years B.P. There are also some reports, as yet unconfirmed, of *Megaloceros* bones recovered from somewhat later Holocene deposits (Vereshchagin and Baryshnikov 1984), and some reports exist of the animal's possible persistence into the Middle Ages in Ireland. I have also observed that a deer depicted in cave paintings at Lascaux may represent the extinct fossil genus *Eucladoceras* (Brodrick 1972: 12).

Numerous accounts also exist of fossil proboscideans surviving into recent times, and of especial interest are those documenting the existence of elephantids (or elephant-like animals) in North and Central America. According to the fossil record, proboscideans first appeared in North America in the middle Miocene, approximately 16 million years B.P., as immigrants from the Old World. They migrated into South America in the Pleistocene with the development of the isthmus of Panama, and their last recorded appearance in both continents dates from approximately 10,000 years B.P. (Agenbroad 1984). Accounts of surviving elephantids include reported



sightings during the last millennium (Eberhart 1983, Lankford 1980), and various archaeological artifacts depicting "elephants" (Farquharson 1879, Elliot Smith 1915). I do not propose to discuss the possible survival of New World proboscideans into recent times in this paper, although it is a fascinating topic that has been much discussed in the literature, and deserves an entire paper in itself.

My intention in this paper is not to review the existing literature on the possible survival of fossil ungulates into historical times, but, rather, to present new examples of archaeological artifacts possibly depicting such. I would like to stress that none of these examples are ones that I set out to discover; on the contrary, they represent instances that have "turned up" in my everyday life, and that were of interest to me because of my research in ungulate paleontology. This suggests that there may be a wealth of further examples of fossil animals represented in archaeological artifacts or artwork awaiting a concerned researcher in cryptozoology. I present here some further examples of possible sivatheres, and examples of artifacts that may depict a chalicotheres, a giant hyrax, and a Pleistocene antelope.

#### SIVATHERES

Sivatheres represent the subfamily Sivatheriinae of the artiodactyl family Giraffidae. The other giraffid subfamily, the Giraffinae, contains two living genera (although it had a much greater diversity in the past), and okapi, *Okapia johnstoni*, and the giraffe, *Giraffa camelopardalis*. Sivatheres are known from the latter part of the Miocene (approximately 15 million years B.P.) through the Pleistocene in both African and Eurasian deposits, and are moderately diverse in the fossil record. In life, they differed from giraffes in being more heavily proportioned, with shorter legs and necks, and had branching, palmate ossicones (cranial appendages that were skin-covered and non-deciduous, in contrast to structures such as naked, deciduous deer antlers, or keratin-covered, non-deciduous bovid horns). Most genera were of similar body proportions to a moose or an ox. They differed from giraffines in the palmation of the ossicones (resembling the condition seen in moose antlers), and the possession of an additional pair of frontal ossicones in front of the main postorbital ones. These frontal ossicones were small structures in the genus *Sivatherium* but were large, and joined together to form a single median structure in the genus *Bramatherium*.

Colbert (1936) described a Sumerian copper chariot ring in the Field Museum of Natural History, Chicago, from Kish, Iraq (3500 B.C.), that depicts a small statuette of an animal (Fig. 1a) that had been previously assumed to be some kind of deer, as it possessed branched, palmated "antlers." Colbert pointed out, however, that these "antlers" bore a close resemblance to the ossicones of *Sivatherium* (Fig. 1b). He also noted a number of

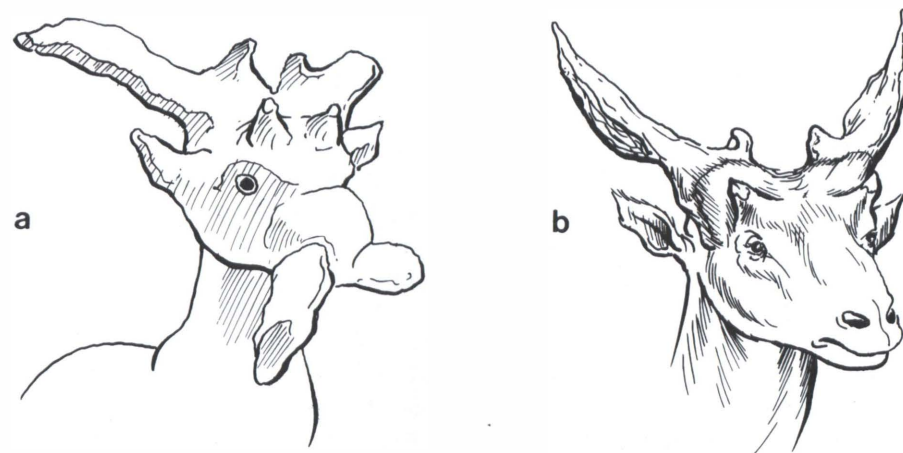


FIG. 1.—a. Statuette on Sumerian copper chariot ring. (From Colbert 1936.) b. Reconstruction of a sivathere head. (From Colbert 1936.)

other sivathere-like features of the statuette, such as the additional pair of small ossicones (which are not seen in any known Recent or fossil deer), and the moose-like short, bulbous snout. Colbert commented that, although the genus *Sivatherium* is known primarily from the Pleistocene of India, it is also known from both India and Africa in association with human artifacts in the Paleolithic, and he suggested that it may have persisted into relatively recent times and may even have been domesticated, as suggested by the representation of a "halter" on the statuette. Savage and Long (1985) also comment on the likelihood of the survival of sivatheres into the early Holocene. In addition to the statuette described by Colbert, Savage and Long present a petroglyph of a possible sivathere from a rock shelter in the Tibesti mountains, central Sahara, dating from approximately 6000 B.C.

A casual visit by the author to the British Museum, London, revealed a surprising number of "sivathere-like" artifacts, most of which could be distinguished by the possession of a single, median cranial appendage resembling the condition in the genus *Bramatherium*. (It is clear, however, that these examples do not represent as strong a case as Colbert's Sumerian statuette.) Although I am aware that these new examples could well represent instances of "artistic license"—and I have not made a study of the types of artistic stylization of the various periods that these examples are from—I should point out that the artifacts were found among others that do accurately depict animals with cranial appendages that were obviously cattle-like horns or deer-like antlers. I present these examples here merely as "additional possibilities" of sivatheres, which gain their strength from the existence of previous, more convincing examples. (As a cautionary tale on relying on



museum identifications of animals depicted in archaeological artifacts, I would like to cite the case of a Roman mosaic in the British Museum that depicts animals termed “deer” that are quite clearly reedbuck antelope, genus *Redunca*, that are unknown outside of Africa!)

In Fig. 2 we see a sketch of the skull of *Bramatherium* on display at the American Museum of Natural History, New York. Fig. 3a depicts a recently made stone statuette, purchased at a Russian tourist shop in 1977, that is apparently of a deer, yet possesses a median, palmated “antler.” Fig. 3b shows an ivory “stag” from Syria (from approximately 200 B.C.), and Fig. 3c shows a bronze “cow” from Egypt (from approximately 600 B.C.), both artifacts on exhibit at the British Museum. Both animals also possess a *Bramatherium*-like medial cranial appendage. In 1974, I examined a book on children’s toys in the Soviet Union (one that I have unfortunately been unable to relocate) that depicted tin whistles from Siberia in the shapes of various animals. These took the form of a ram, a goat, and a “deer,” but while the horns on the ram and the goat were realistically portrayed, the deer possessed a median, palmate “antler” resembling the animal depicted in Fig. 3a. While I would not vigorously dispute the interpretation that these recent artifacts merely represent a conventionalized styling of deer antlers, and would not venture to suggest that *Bramatherium* survives today in the Soviet Union, the possibility remains that the stylized convention reflects a folk memory of an animal with cranial appendages of this type that existed in the area until relatively recently.

#### CHALICOTHERES

The family Chalicotheriidae belongs in the order Perissodactyla, and contains taxa that were mainly of large (cow to giraffe) size. Chalicotheres are known from the Eocene (approximately 45 million years B.P.) through the middle Pleistocene over most of the Northern Hemisphere (although they became extinct in North America in the Miocene, approximately 15 million years B.P.). They were rather horse-like in appearance—especially the head—but had secondarily substituted large claws for hooves, which they probably used to pull down tree branches to within access (Coombs 1982), and had a somewhat hyaena-like posture, with the front legs longer than the back ones (Fig. 4). Two main forms (subfamilies) of chalicotheres existed: the schizotherines, that were more horse-like in proportions, with longer hind legs and a longer face; and the chalicotherines, that were more hyaena or gorilla-like in proportions, with longer fore limbs and a shorter face. Both schizotherines and chalicotherines were found in the Old World until the middle Pleistocene, but only schizotherines were found in North America (Coombs 1982). The chalicothere depicted in Fig. 4 is based on the genus *Moropus*, a schizotherine from the middle Miocene of North America.

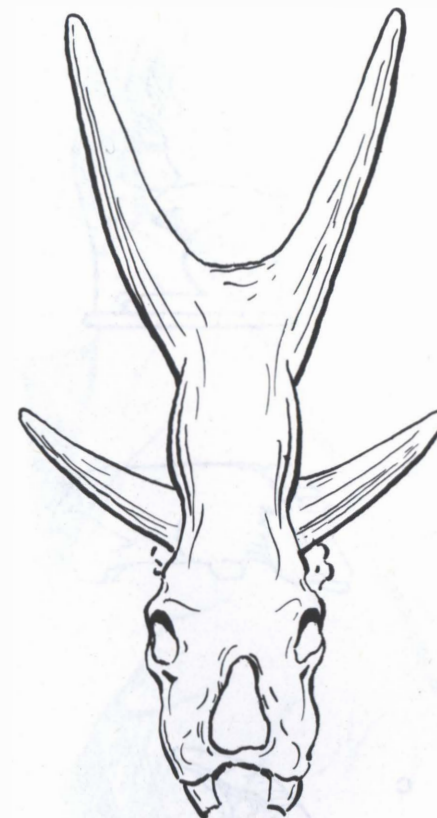


FIG. 2.—*Bramatherium* skull (AMNH 32603) on display at the American Museum of Natural History, New York.

Heuvelmans (1958) made the suggestion that a surviving chalicothere might be behind the East African legend of the “Nandi Bear” or *chemosit*. The chalicothere certainly agrees with the visual descriptions of the Nandi Bear, a large, stocky animal with high shoulders and a sloping back, no visible tail, and a rather long, pointed snout, that occasionally stood up on its hind legs. The Nandi Bear was also variously described as possessing a hairy mane, which is typical of certain living perissodactyls (such as horses and American tapirs). It seems unlikely, however, that a herbivorous chalicothere could have been responsible for the type of ferocious killings ascribed to the Nandi Bear, and Heuvelmans suggested that such incidents might actually have been the work of some other local animal, and then ascribed to the occasionally glimpsed chalicothere—which may have been *assumed*.



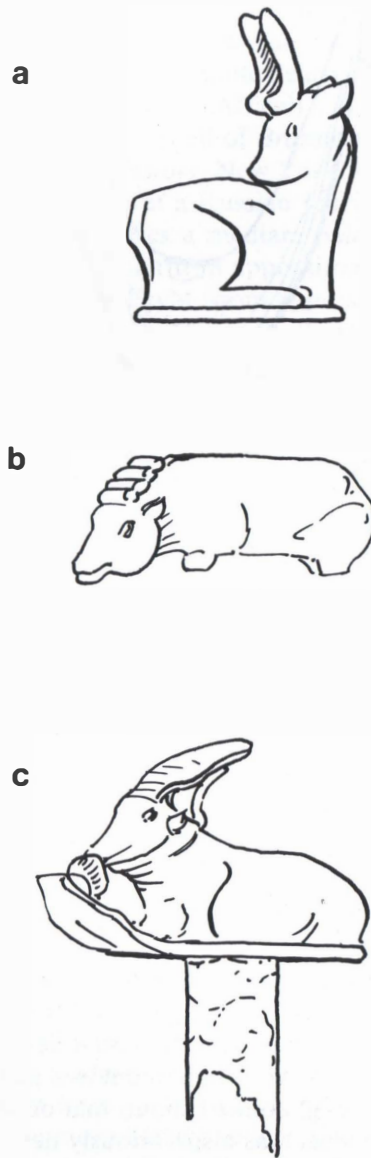


FIG. 3.—a. Stone statuette from Russia (recent). (Property of author.) b. Ivory “stag” from Syria (from approximately 200 B.C.) on display at the British Museum, London. c. Bronze “cow” from Egypt (from approximately 600 B.C.) on display at the British Museum, London.



FIG. 4.—Representation of a chalicothere, based on the North American Miocene genus *Moropus*.

to be carnivorous due to its large size, impressive claws, and hyaena-like posture.

Martin Pickford, a British paleontologist, also commented on Heuvelmans' idea of equating a surviving chalicothere with the Nandi Bear (Pickford 1975). When Pickford was working in Kenya on the description of a new chalicothere fossil (ascribed to the species *Ancylotherium hennigi*) from the Lukeino formation (African Miocene, approximately 6.5 million years B.P.), he verbally described what the living animal might have looked like to his Kenyan field assistant. Curiously, the assistant assured him that this animal was just like a *chemosit* (the original native name for the Nandi Bear), an animal well known by repute, and this view was later repeated by other local Kenyans. Other paleontologists have also commented on the possibility of a chalicothere surviving in Africa until at least relatively recent times (Andrews 1923, Leakey 1935, Savage and Long 1985). However, Pickford's *Ancylotherium* is a schizotherine chalicothere, and it has generally been assumed that, if the Nandi Bear reports were indeed attributable to a chalicothere, it would be to a chalicotherine type, as this animal would better fit the description of bear- or hyaena-like proportions (Heuvelmans 1958). The chalicotherine genus *Chalicotherium* is known only from the middle Miocene of Africa, and the chalicotherine genus *Nestoritherium* is known from the Pleistocene of Eurasia.

My own evidence for a chalicothere surviving into historical times comes not from Africa, but from the frozen tombs of Siberia (the Sakic culture, dated as 500–400 B.C.). Zavitukhina (1978) illustrates two belt plaques from the collection of Peter the Great in the Hermitage, Leningrad, depicting “fabulous wolf-like animals” (Fig. 5). However, this animal looks surprisingly chalicotherine-like, as it appears to combine a horse-like head and body (including the presence of a horse-like mane) with clawed feet. In contrast to the type of chalicothere assumed to be behind the reports of the Nandi Bear, the proportions of this animal suggest a schizotherine (or horse-like) chalicothere. Schizotherines were not known beyond the end of the Miocene in Eurasia, but *Ancylotherium* survived in Africa until the middle Pleistocene. Additional features include the suggestion of a small proboscis (the retracted nasal bones of certain chalicotheres suggest that they might have possessed a tapir-like proboscis), and a long tail (which was not a feature of chalicotheres). While this animal could represent a stylized wolf, other belt plaques from the same collection depict animals that are clearly dogs or foxes, with pointed muzzles, triangular ears, no mane, and a tufted tail—all in contrast with this supposed “wolf.”

The main problem with the interpretation of this animal as a possible chalicothere is the presence of a long tail. However, the animal does not bear a resemblance to any known fossil or living carnivore, and its general

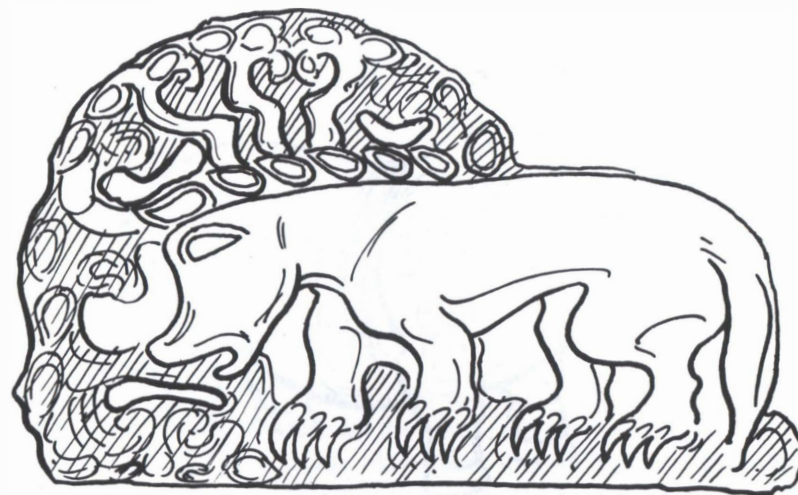


FIG. 5.—Gold belt plaque of “fabulous wolf-like animal” from the frozen tombs of Siberia (Sakic culture, 500–400 B.C.), in the collection of Peter the Great in the Hermitage, Leningrad, U.S.S.R. (Si 1727 1/19). (Drawn from photograph in Zavitukhina 1978.)

appearance suggests the depiction of a horse with claws. Perhaps the animal was not directly known to the artist, but had survived in folklore as an animal with “a horse-like head and body, but with wolf-like claws,” which could account for the reconstruction of the animal with a long horse-like tail. Again, my role here is as a paleontologist, and I am not in a position to discuss possible artistic styles or the animal folklore or mythology of the period. The best that I can say is that it looks like a chalicothere to me! Once again, the argument is strengthened by other accounts of possible surviving chalicotheres.

These “surviving chalicotheres” examples are weakened by the available fossil evidence, inasmuch as schizotherines were the surviving African Pleistocene chalicotheres, while chalicotherines were the Eurasian Pleistocene forms, yet these examples suggest an Asian schizotherine and an African chalicotherine! Chalicotheres are rare as fossils, however, and in more recent (post Miocene) sediments they are usually identified only by isolated elements. Moreover, land connections between Africa and Eurasia have existed since the middle Miocene, and both types of chalicotheres *did* survive in the Old World in the Pleistocene. Thus, it is not impossible, in terms of paleobiogeography, that, in more recent times, a schizotherine chalicotherine species could have migrated into Asia, and a chalicotherine chalicotherine species could have migrated into Africa, even if one would like the fossil record to be more supportive of all this cryptozoological evidence. In con-



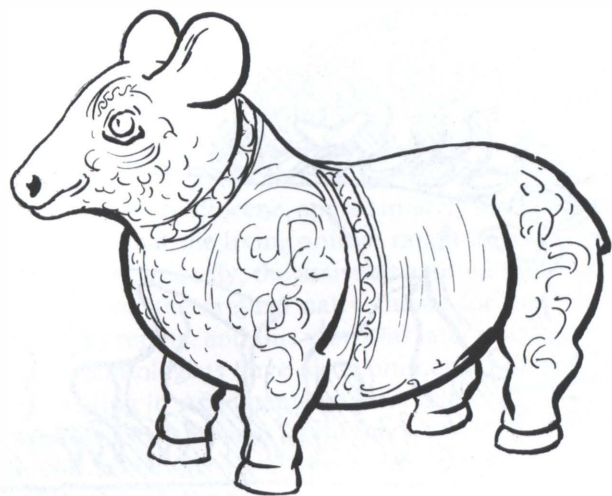


FIG. 6.—Bronze statuette of hyrax-like quadruped from the late Chou Dynasty (600–500 B.C.), China, on display at the Freer Gallery of Art, Smithsonian Institution, Washington, D.C. (From reproduction cast, property of author.)

trast, a report of a “surviving chalicothere” in South America would be an extremely dubious claim, as they are not known from that continent at all; they are believed to have become extinct in North America long before the formation of the Panamanian land bridge linking North and South America.

#### A GIANT HYRAX

In Fig. 6, we see a bronze statuette from the Freer Gallery of Art (Smithsonian Institution), Washington, D.C., from the late Chou Dynasty of China (600–500 B.C.). Several of these statuettes were found in 1923 near the village of Li-yu, in the Shanxi Province. An issue of *Smithsonian* magazine once described this animal as follows: “Fanciful tapir-like beast (that) seems to have had no practical or ritual function.” The artifact depicts an animal that appears to combine the head of a hyrax with the body of a hippo, but possessing horse-like single hooves and a short, hippo-like tail (the tail is lacking in living rock hyraxes, but a small tail is present in the tree hyrax, and may have been present in extinct taxa).

Hyraxes are known today from Africa and Asia Minor, but in the Pliocene (5 to 2 million years B.P.) they were widespread across southern Eurasia, and they exhibited a much greater diversity of body size and form than is seen today. A recent Asian rock hyrax, *Procavia habessinica* is depicted in Fig. 7. Of special interest is the existence of a (relatively) giant hyrax from the late Pliocene (Villafranchian) of China, *Pliohipyrax*. An undescribed skull of this animal from the collection of the American Museum of Natural



FIG. 7.—A recent rock hyrax (*Procavia habessinica*).

History is shown in Fig. 8. This skull is approximately 25 cm in length, and the animal would have been about the size of a large pig (present day hyraxes are rabbit-sized). It also seems likely that this animal was semi-aquatic. Although no post-cranial remains are known, the high position of the orbits is reminiscent of that condition in the hippo, and it is also seen in other fossil ungulates that are known to have had hippo-like bodies and dense bone typical of semi-aquatic animals (for example, the Miocene rhinoceros *Teleoceras*). The relatively high position of the orbits in the skull of the hippo places the eyes near the top of the head, enabling the animal to rest in the water almost fully submerged but with the eyes uncovered. Similar adaptations can be seen in other semi-aquatic animals, such as crocodiles.

*Pliohipyrax* appears to be a likely candidate for the Chinese sculpture. The head of the statuette is clearly hyracoid, and the head of *Pliohipyrax* (Fig. 8) shows similar proportions to a living hyrax. The head of the statuette shows no resemblance to a tapir, as it lacks a proboscis-like snout, and is unlike a hippo in the large size of the ears. Additionally, the hippo-like body proportions of the statuette suggest a semi-aquatic mode of life. The limb stance is more pillar-like than in living hyraxes, but that would be expected in a larger-sized animal. I would suggest that the statuette was a representation of an animal known from mythology, rather than one personally seen by the artist, as the horse-like single hooves are anomalous.

#### A PLEISTOCENE ANTELOPE

A reproduction of an African Tji Wara, or agricultural antelope god, appears in Fig. 9. The country of origin is unknown, but the style suggests East Africa (probably Kenya) rather than West Africa. Most other Tji Waras that I have seen clearly represent a stylized sable antelope, *Hippotragus niger*, recognizable by horns that are convexly curved and that project backwards without lateral divergence. The form of the horns on this artifact bears no close resemblance to any known African antelope, but it does resemble a longer version of the type of horn seen in the Indian nilgai, *Boselaphus*

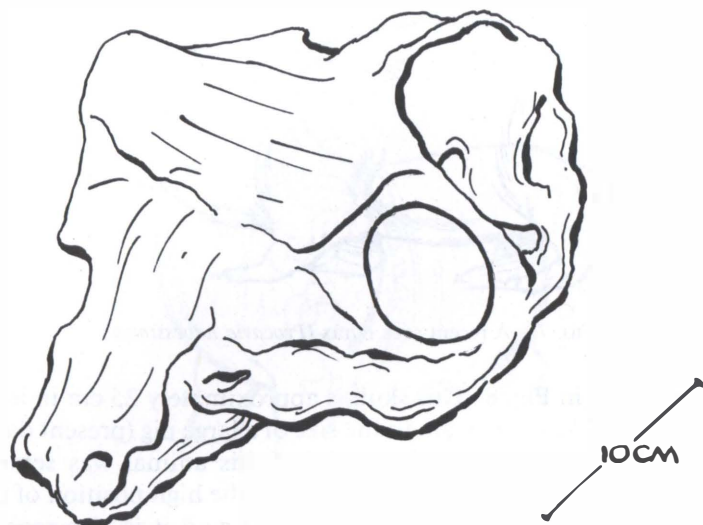


FIG. 8.—*Pliohipraxis* skull (AMNH 99375) in the collections of the American Museum of Natural History, New York.

*tragocamelus*. The living African antelope which might fit this representation are the oryx, *Oryx gazella*, in which the horns may project slightly laterally but are more convex in outline, and the waterbuck, *Kobus ellipsiprymnus*, in which the horns have a stronger lateral projection and are more definitely concave. However, both of these living antelope have broad muzzles. The narrow muzzle seen in this sculpture is more reminiscent of the head of the nilgai.

Interestingly enough, a close relative of the nilgai, the genus *Mesimbroportax*, existed in Africa during the Pleistocene, and this animal had horns that were identical to those portrayed in this artifact. Again, while I would not categorically assert that this artifact *must* represent a surviving memory of *Mesimbroportax*, I find it intriguing that the resemblance is so close, and that the animal bears no obvious resemblance to any living African antelope species, in contrast to all the other examples of Tji Waras I have seen.

#### CONCLUSIONS

With the possible exception of the giant hyrax, none of these new descriptions carry quite the strength of Colbert's (1936) description of *Sivatherium*.



FIG. 9.—Iron cast reproduction of an African Tji Wara. Original probably in wood, and from Kenya. (Property of the author.)



Yet it is possible that various fossil animals survived into at least recent enough historical times to have become part of folklore, and thus be depicted in archaeological artifacts, despite the fact that their fossil record does not extend into the Holocene. (Numerous examples exist of presumably extinct animals with fossil records that terminated a considerable time ago, but which have then been found alive. The best known example is that of the coelacanth fish *Latimeria*. Coelacanths are not found in the fossil record beyond the end of the Cretaceous period, 65 million years B.P.) The significance of such artifacts is more likely to be noted by paleontologists than by archaeologists because of their familiarity with animals represented in the fossil record. This is particularly true, for example, in the case of the giant hyrax, as this animal has never even been depicted in the semi-popular literature.

My personal opinion on this entertaining catalogue of artifacts is as follows. The "sivatheres" presented here may merely represent stylized deer, yet Colbert's (1936) description of the probable *Sivatherium* statuette is highly convincing, as is the petroglyph depicted by Savage and Long (1986), and this lends some credence to my contention that *Bramatherium* might also have persisted into relatively recent (historical) times, and may be represented in at least some, if not all, of these artifacts. The "chalicotheres" is problematical, especially in its possession of a tail. However, oral traditions and eyewitness reports of possible chalicotheres exist in Africa, and I am not convinced that the Siberian plaque depicts a wolf, if only for the reason that other canids (dog-related animals) are accurately depicted in the same collection. As to the giant hyrax, as someone who has kept hyraxes as house pets for five years—and who can thus only too well recognize a hyrax on first sight—I am fairly convinced that the Chinese statuette is truly a representation of *Pliohyrax*. My only regret is that the horse-like hooves imply that the animal was probably fashioned from folk memory, and that mounting an expedition to China to look for it would probably be futile! Finally, the Tji Wara sculpture may well represent an oryx or a waterbuck, yet the existence of a Pleistocene antelope in Africa that so closely resembles this artifact is intriguing.

#### ACKNOWLEDGMENTS

I would like to thank Adrian Lister for providing me with information on the Irish elk artifacts; Richard Tedford for providing me with the correct identification of the American Museum of Natural History giant hyrax skull; and Richard Greenwell, Michel Raynal, Karl Shuker, and Michael Swords for their correspondence, and for their bringing certain literature to my attention. Thanks also go to Brian Regal for the illustrations, and to two anonymous reviewers who commented on the first draft of this paper.

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## A RECONSTRUCTION OF THE SKULL OF *GIGANTOPITHECUS BLACKI* AND ITS COMPARISON WITH A LIVING FORM

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**ABSTRACT:** A skull of *Gigantopithecus blacki* has been constructed by the author based on the adult male jaw from China. This presumes erect, bipedal locomotion for the original, and an ape-sized brain scaled to the appropriate body size. The final product is far larger than a male gorilla, and compares favorably with reports of the unverified North American Sasquatch (Bigfoot).

### INTRODUCTION

The largest known primate, *Gigantopithecus blacki*, lived in southern China somewhere between a half and one million years ago. It is known from the tooth-bearing parts of three lower jaws and over a thousand loose teeth. Dental traits clearly show it to be hominoid (in the superfamily of apes and humans), but authoritative opinion is divided as to whether or not it should be classified within the human family, Hominidae.

Upright, bipedal locomotion is the ultimate criterion for inclusion in the human family, as opposed to that of the great apes, Pongidae. Weidenreich (1946) and Eckhardt (1972) classed *Gigantopithecus* as a hominid, and proposed it as an actual human ancestor, but were vague on the point of bipedalism. Wu (1962) and Robinson (1972: 6) made the species hominid and bipedal, but on a separate line from ourselves, and presumably extinct.

As far back as the early 1950's, Heuvelmans (1952) suggested, on the basis of the few teeth then known, that *Gigantopithecus* might be the direct ancestor of the reported Himalayan Yeti. Later, Sanderson (1961) and Coon (1962: 207) also speculated on its possible relationship to the Yeti.

In 1968, Green made the connection with the North American Sasquatch, or Bigfoot. More recently, I formally proposed to equate the two species under *G. blacki*, but expressed the hope that *G. canadensis* would become the accepted name if the Sasquatch proved to be a separate species (Krantz 1986). I also suggested using the name *Gigantanthropus canadensis* if it should prove to be generically distinct, or *Australopithecus canadensis* if future discoveries should point in that direction. However, such proposals carry no legal weight under the established rules of zoological nomenclature.

All of these ideas presently suffer from the fact that there is no direct overlapping of evidence between the known fossils and the reported living species. We have no footprints or eyewitness accounts of the Chinese fossil

animals; we have no skeletal remains of the North American Sasquatch, or of any other unverified hominoid.

Pilbeam *et al.* (1980) allowed about 100 kg body weight for the possibly related fossil jaw from India. Simons and Ettel (1970) assumed all gigantopithecines were knuckle-walkers. Helmut Hemmer (personal communication, 1985) questioned our ability to make any predictions of body size or locomotion from dentition. If all or any of these opinions are correct, we have little reason to seriously equate *Gigantopithecus* with the Sasquatch, or with any other of the reported unknown hominoids.

Contrary to the common view, I think a great deal can be determined about body size and locomotion from dentition alone. This depends on correctly identifying the type of teeth and how they were used. But with *Gigantopithecus* we also have much of the jaw itself, thus greatly improving our predictive ability from that based on teeth alone.

With some tooth-to-jaw ratios in hand, it should not be difficult to reconstruct the entire mandible. Extant upper teeth can be fitted, with supporting bone, to restore much of the upper jaw directly. The necessary muscle crests and other supporting structures follow almost automatically. Given the great divergence of the jaw, as well as the reduced sectorial complex in the teeth, an erect posture may be deduced (Krantz 1981). This tells us how to orient the muscle crests at the base of the skull. The overlap of *Gigantopithecus* with early *Homo* allows us to eliminate the former from consideration for cultural behavior, and thus predict ape-like cognitive abilities. Scaling up from the chimpanzee at 400 cc of braincase volume to the gorilla at 500 cc, one can reasonably assign 600 cc to this fossil form. A full reconstruction of the skull thus should offer no serious problem. Such a reconstruction would give strong indications of the total body size and general appearance of these animals in life. And this, in turn, might allow for some more direct and perhaps meaningful comparisons with the reported living form.

### THE RECONSTRUCTION

Having made the above observations many times, I have been urged repeatedly to put my hands where my mouth is, and actually produce this reconstruction. During the autumn of 1985 I made the mandible, and in the spring of 1986 the rest of the skull was completed. What follows is a description of the more detailed reasoning and procedures that were employed in this work.

I began by moving the right third molar laterally from its obviously disturbed position to line up the tooth row like the undistorted left side. The right second molar and the two medial incisors were added, and a few surface irregularities were filled in. None of this required any serious judgment, much less reconstructive imagination.



The first step was to establish the most likely breadth of the ascending ramus of the mandible. Simply stated, this means extending a line along the lower tooth row beyond the last molar to where it meets the upright, rear edge of the jaw. I measured the actual breadth of this ascending ramus, at the level of the tooth row, in my available sample of hominid and pongid jaws. This measurement was then laid out along the length of the tooth row in each specimen, from the back of the last molar, to see how far forward it went.

The most common result was that the ramus breadth equaled the distance from the end of the tooth row to the middle of the anterior premolar. This was true for such specimens as the Upper Cave Zhoukoudian male, the La Ferrassie Neanderthal, one robust australopithecine, a male gorilla, and a male orang-utan. The Heidelberg jaw had a greater ramus breadth, equal to a line running just onto its canine. Other jaws had slightly narrower rami, such as Cro-Magnon, Skhul V, male "Sinanthropus," three robust australopithecines, and a female orang-utan.

Since the existing part of the *Gigantopithecus* jaw shows excessive height and thickness relative to its teeth, I assumed that its length (ramus breadth) might lean in this same direction. Accordingly, I chose the slightly above-average measurement basis of mid-anterior premolar to last molar. This was 88 mm—well above the largest of my comparative samples (75 mm in a male gorilla), and twice the average for specimens within the genus *Homo*.

The front edge of the ascending ramus, the coronoid process, is preserved in its lowest part on each side. Since this edge runs almost straight up (perpendicular to the occlusal plane of the teeth), it provided the anterior edge of the 88 mm wide ramus (Fig. 1).

The two rami are set far apart and continue to diverge strongly toward the back. This divergence follows the approximate trend of the molar-premolar row, and also the direction of the lower edge of the mandibular body. It is normal in most primates—and in all specimens in my hominoid sample—for the jaw to continue this divergence in a nearly straight line along each side (Fig. 2).

Reconstructing the height of the ascending ramus followed a similar procedure. The total ramus heights to the condyles were measured perpendicular to the surface on which the sample jaws rested. The heights from the crowns of the third molars to the same resting surface were then measured. (I measured from the center of the molar crown to neutralize most wear differences.) Seven *Homo* specimens clustered tightly around the mean of ramus height, being 1.64 times M3 + body height. Four robust australopithecines clustered tightly around 2.0 times taller. Three apes ranged widely around 2.08 times taller.

I made the *Gigantopithecus* ramus exactly 1.96 times taller (199 mm); I was aiming for 2.0 times, but the clay slumped slightly. This height also followed from trying to visualize the condyle's vertical position in order to

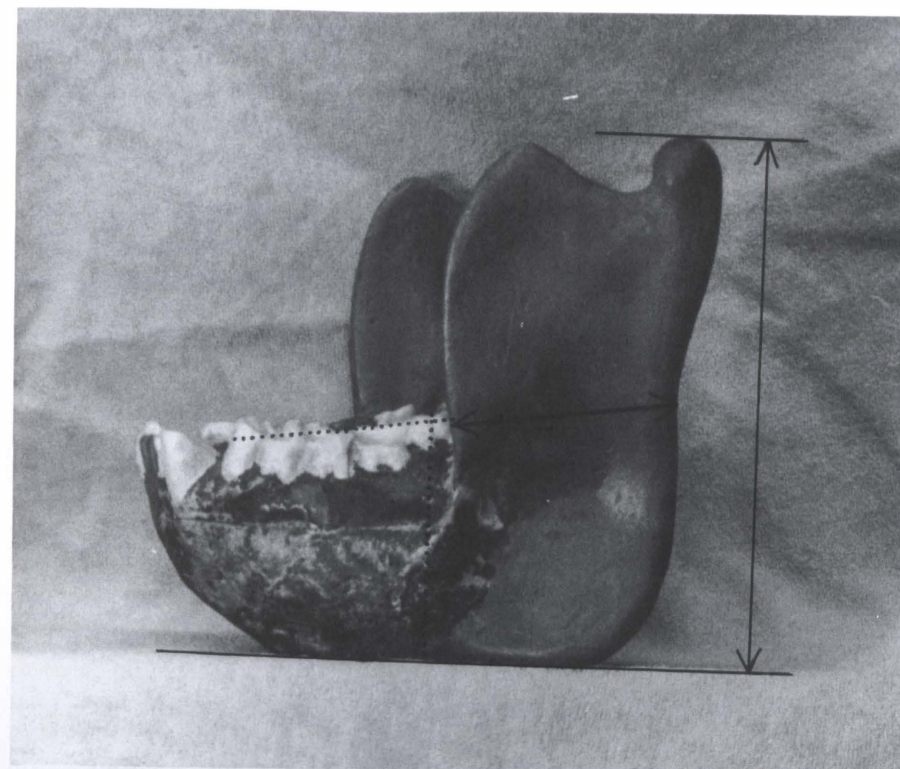


FIG. 1.—Side view of reconstructed jaw, with flat surface to camera. The 88-mm breadth of the ascending ramus (rear, rising part) is based on the length along the tooth row shown in dotted line. The 199-mm height of the ascending ramus, from resting surface, is twice the height at the third molar, shown as another dotted line.

give the jaw the motions that would produce the observed tooth wear. Here, the cuspal interlocking is well worn away by an anterior migration of the lower jaw. This requires a highly placed condyle, well above the occlusal plane, so that the jaw moved forward as well as upward as the tooth crowns eroded in life (see Fig. 1).

After settling on the breadth and height of the ascending ramus, I was able to model the details with typical hominid contours. There were some guides in the extant fossil jaw. The divergence of the body of the mandible was continued to the rear corners (gonia), then thickened with a slight outward flare as in normal hominoid jaws. This gave an excessive breadth, but any attempt to narrow it would have produced an unusual shape with no justification.

The leading edge of the ascending ramus (coronoid process) is preserved enough to show it also has a strong outward flare. This obliged me to re-



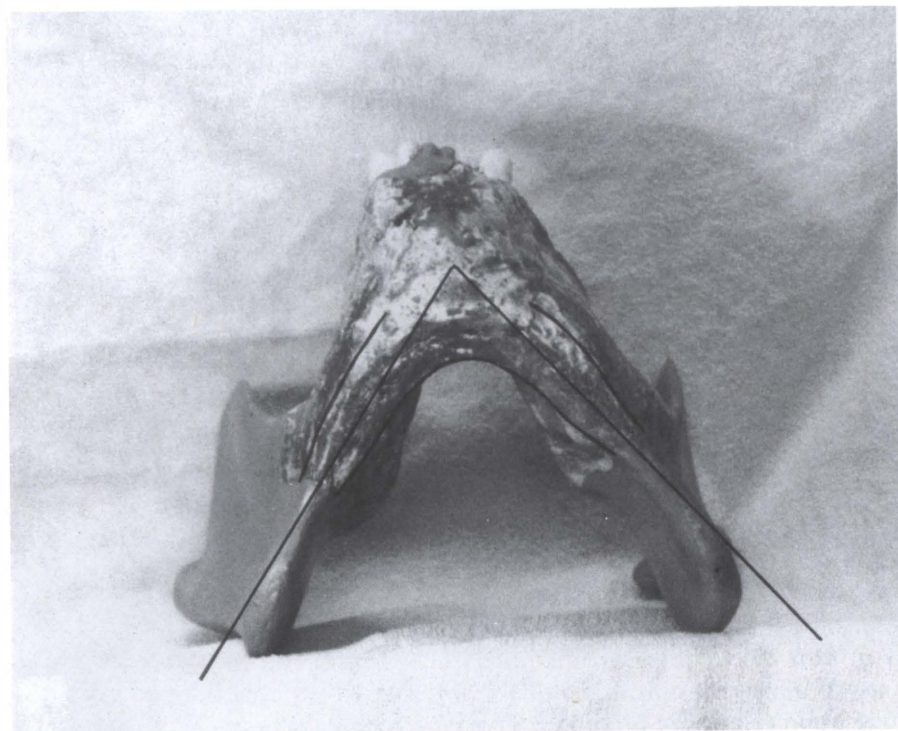


FIG. 2.—Bottom view of reconstructed jaw. The basal portion is outlined, and the general trend of divergence is drawn in as straight lines. These spread at  $76^\circ$ , while an earlier reconstruction by the author put this at  $84^\circ$  by concentrating more on the lowermost edge. The posterior parts follow this trend but are inflected slightly at the corners.

construct a considerable concavity in the lateral surface of the ramus. This implies a very powerful masseter muscle, and, in turn, very strong zygomatic arches (cheek bones) above, where the masseter originates.

The size of the condyle is proportional to the jaw. Its shape, and the entire outline of the ramus as well, follows a generalized hominid/hominoid design. Relative thickness of the reconstructed bone is greater than is usual in primates. This is an allometric effect, as relative thickness increases with absolute size in extant forms, so this is simply extrapolated a considerable step beyond the gorilla.

In all this, as well as other parts of the skull, I first made numerous drawings and partial restorations showing the new parts both larger and smaller than what was ultimately decided on. Each of these extremes was then studied in order to determine if the implied anatomical relationships were unworkable, inefficient, or wasteful of biological material. After bracketing with too large—too small, too wide—too narrow, too tall—too short, etc., I was able

to close in on what appeared to be the most likely size and/or shape. This involved placing all muscles, locating all levers, judging all moving masses, and so on, for all normal as well as abnormal movements of these parts.

I would not be so bold as to suggest this procedure was perfect and yielded exact results, but it is not likely that any serious error could have been introduced. Of course, only the finding of an actual *Gigantopithecus* skull will serve to check its accuracy.

This mandible might now be compared with the earlier reconstruction by Wu Rukang (1962). Our differences are only of degree. Wu made the ramus somewhat less tall, but prolonged it backward much more than I did. He seems to have tried to turn the sides inward slightly to avoid some of the indicated divergence, and thereby give it a more “normal” appearance. By giving it such length, however, his jaw is actually wider at the back than mine is. Overall, Wu’s reconstruction is somewhat the larger and more ape-like. A more serious comparison of actual measurements is saved for later in this paper, but it is fair to say at this point that a 500-kg body size is quite possible.

The upper dentition is based on Wu’s description of the recovered teeth. These fall into two size categories, the larger of which correspond to those in the mandible and are presumed to be of males. Average measurements of these larger teeth were laid out in an arch that would normally occlude with those in the lower jaw. This involves having the upper teeth overlap outside the lowers around most of the arch, but close in to meet just about crown-to-crown at the third molars. The observed wear pattern and its slopes follow from this occlusion, which is also true for almost all primates.

In constructing this upper dental arcade, I was unable to fit in teeth of average male size, but had to reduce them slightly. This implies that this individual was somewhat below average size—at least in its dentition.

The alveolar (tooth-supporting) part of the upper jaw was made relatively thick, corresponding to its thickness in the lower jaw. The hard palate was placed at a medium height, typical of early hominids. This may not be correct, but minor variations in this have little repercussion on any other traits. (The reader might note that the restorations are based on hominoids when apes and humans are the same, and on hominids when there is a difference.)

Recovered upper canines have long roots (45 mm). This would place their supporting alveolar bone well above the lower edge of the nasal aperture. The distance between the upper canines thus strongly influences the breadth of the nasal aperture (Coon with Hunt 1965: 250). The spacing between these canines can be determined from the sizes of the four incisors that form an arc between them, and by the positions of the lower canines which the uppers should partially overlap. These two methods agree in placing the canine roots’ medial edges about 30 mm apart at the gum line, and spreading



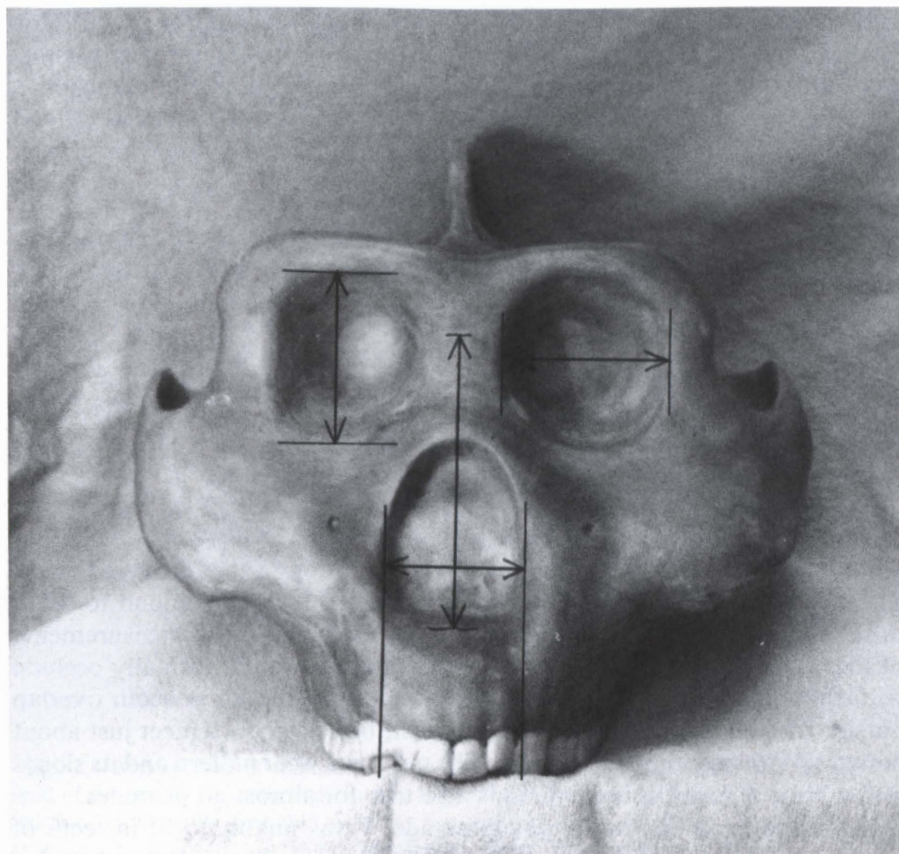


FIG. 3.—Front view of reconstructed cranium. The 50-mm nasal breadth lines up with the spacing between the canine tips. The 115-mm height of the nasal chamber is slightly arbitrary at the top, but corresponds well with the great height of the mandible. Orbit diameters of 65 mm are also indicated.

to at least 50 mm at their tips. This allows a lower breadth of nasal aperture also of about 50 mm, and higher up it can have little or no greater breadth (Fig. 3).

The height of the entire nasal chamber can be roughly deduced from its breadth and from the total body size. The volume of inspired air must be proportional to the body's needs. With a nasal chamber breadth somewhat more than that of a male gorilla, and an all-over body at least twice as great, the same level of activity can be maintained with a chamber half-again taller. This kind of nasal height (115 mm vs. 88 mm in the gorilla) was introduced into the reconstruction, and was found to fit nicely with the high placement of the mandibular condyles. This internal cross-checking was gratifying (see Fig. 3).



FIG. 4.—Top view of reconstructed cranium. The 30-mm flange of the occipital crest is indicated near the top of the picture. The notable narrowing, or postorbital constriction, is indicated near the center, beyond which are the great spaces available for the temporal muscles on each side.

The braincase was built from a core of plaster—a cast of an australopithecine skull of 510 cc—which was thickened somewhat to approximate a 600-cc capacity with a presumed vault thickness of 6–7 mm. This was placed between the mandibular condyles, about balanced in front and behind them, and elevated so that about one-tenth of its height was below the condyles' tops. This is the standard brain location in hominoids in relation to the condyles, with almost no variation.



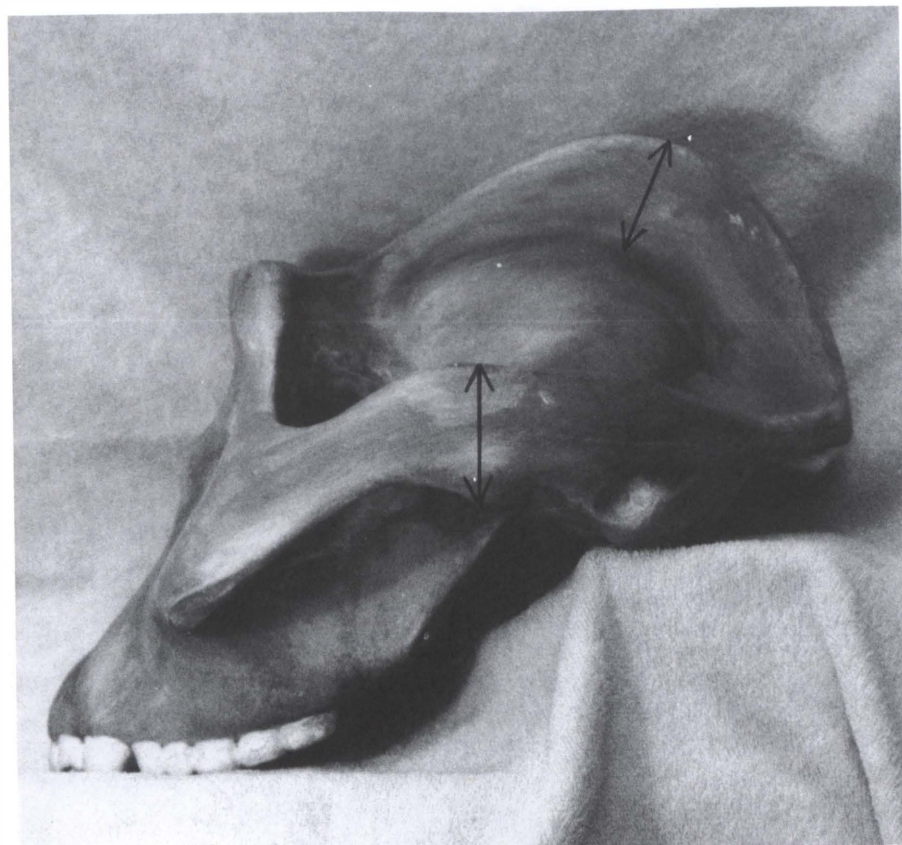


FIG. 5.—Side view of reconstructed cranium. The 50-mm extension of the sagittal crest is indicated near the top of the picture. Vertical thickening of the zygomatic arch is shown nearer the center.

From the sides of the braincase I built up typical mandibular sockets (glenoid fossae) that reached out and capped the condyles. From the lower front part I added material to join with the palate and growing nasal chamber that had already been constructed. This formed a basic frame upon which I could then add all other superstructures.

At the base of the braincase, from one jaw socket around the back to the other socket, I built a horizontal crest standing out about 30 mm (Fig. 4), and typically 15 mm thick (Fig. 6). This constitutes the supramastoid crest in each ear region, and the occipital crest across the back. The size of this crest was carefully calculated for leverage for the muscles of the neck that would have inserted below it in an erect body. The size and shape of this structure was further refined as the rest of the reconstruction progressed. If

this errs, I think it is on the conservative side; it could have been made more prominent.

Orbit size was extrapolated up from the gorilla, much as was done with brain volume. I made cones of plaster, gradually enlarging them until they looked about right—inside diameters at rims of 65 mm in both directions, and with depths also correspondingly greater than in the gorilla (Fig. 3). Their thickness, including brow ridges, was again scaled up from the gorilla as would appear to have been necessary to avoid breakage from rough handling in life. Considering the inertial mass that emerges, such bony strength would have to be very great. Again, I may have underestimated the strength of the structures involved.

The orbits had to be placed well forward of the cheeks in order for them to clear the coronoid processes of the mandible. The evidently vertical emplacement of the upper incisors and canines argued for a nearly vertical upper face, also putting the orbits well forward. Vertical placement of the orbits was roughly dictated by the nasal cavity that had already been built. At this level, the optic foramina would enter the braincase at the anatomically correct place. Again, there is internal cross-checking of the design.

Having built and placed the orbits in relation to other facial parts, I then found they were extraordinarily far forward from the braincase (Figs. 4 and 5). Likewise, the back of the upper jaw was also well ahead of the braincase, in addition to being placed far below it. In effect, the apparent joining of facial to cranial parts was reduced to remarkably slender connections. I therefore felt obliged to arbitrarily add extra thickening to much of this connecting area, and to introduce something of a diagonal brace from the lower front of the braincase to the back of the upper jaw. The position of this brace was determined for maximum strength and minimum interference with muscular actions in this area.

Still, the depression left between face and braincase remained impressive (see especially Fig. 4). The more modest development of this same depression in other skulls is known as the temporal fossa. It contains the anterior portion of the temporal, one of the major chewing muscles. It does not automatically follow that the fossa was entirely filled with temporal muscle; however, given the power needed to move a jaw of this size, such a large temporal muscle may well be expected.

The temporal muscles, in all mammals, originate on the sides of the braincase, pass forward and downward under the zygomatic arches, and insert in the coronoid processes of the mandible. In this case, as with the larger apes, the muscle is so large relative to the brain that it covers the braincase entirely and originates from a sagittal crest. This crest extends above and behind the braincase in the midline. Its prominence appears mainly to provide for length of action of this muscle rather than for area of attachment.



As a given type of animal is increased in size, jaw muscles increase in proportion to the total body, while the braincase increases only slightly. Smaller apes lack sagittal crests—the relatively large braincase providing sufficient attachment for the temporal muscles. Male orang-utans and female gorillas often have slight crests, while the male gorilla regularly shows a prominent one. The absolute size of both sexes of *Gigantopithecus* evidently exceeds the male gorilla, and would be expected to have correspondingly larger sagittal crests. In the reconstructed male specimen here, a crest standing 50 mm tall, twice that of the gorilla, is thus a reasonable expectation (Fig. 5).

In gorillas, the major projection of the crest is toward the rear of the braincase; in *Australopithecus*, when it occurs at all, it is more at the top of the skull. Given the hominid design of the skull being reconstructed, the upper emphasis might be expected (Tim White, personal communication). On the other hand, the recently discovered earliest example of *A. boisei* has a more posterior emphasis to the crest (Walker *et al.* 1986). I designed the *Gigantopithecus* crest, on the basis of an indefinable “feel,” to have this same posterior emphasis. The accuracy of this detail remains uncertain.

Gross restoration of the zygomatic arches was simple. They arose above the first molar, as in most hominids, swung around well clear of the coronoid processes, and joined the base of the skull above the condyles and ears to merge into the occipital crest. Their thickness and height, at least anteriorly, posed no problem, though a somewhat greater size and lateral flare might have been added (Allan Smith, personal communication). The masseter muscles, running from the underside of this part of the arch down to the angle of the mandible, were evidently powerfully developed. This means that the arch itself must be strongly built to resist their downward pull.

Reconstructing the rear part of the zygomatic arch posed an unexpected problem when it was observed that the jaw sockets were much more widely spaced than the sides of the braincase. In chewing, a considerable force is exerted upward through the condyle and into its socket, and which must then be dissipated into the skull. In the human skull, the side wall of the braincase passes down just to the outside of this socket, and thus braces the socket against upward displacement. In the gorilla skull, the side wall of the braincase passes down just to the inside of this socket, and thus likewise braces the socket against upward strain. In the *Gigantopithecus* skull, the side wall of the ape-sized braincase passes down some distance medial to the inside edge of the wide-set condyle, thus affording almost no direct resistance against upward strain (Fig. 6). The occipital crest of the gorilla, passing upward and backward from the socket, adds considerably to its bracing. But in *Gigantopithecus*, the horizontal occipital crest offers almost no such bracing.

Some structural support for the jaw socket was achieved by thickening

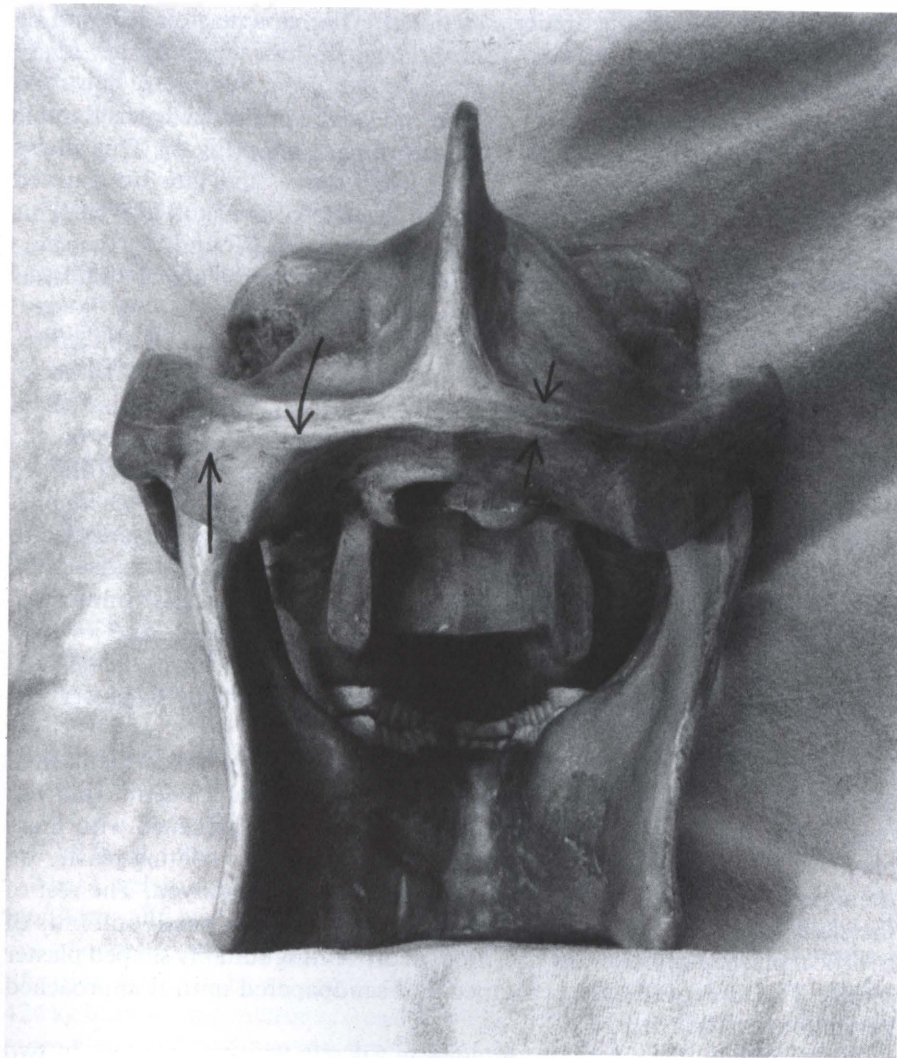


FIG. 6.—Rear view of reconstructed skull. The 15-mm thickness of the projecting occipital crest is indicated on the right between the short arrows. On the left are shown the opposing forces of jaw pressure and cranial-wall resistance which conspicuously mismatch.

the lower braincase wall, and making it spread out widely just above the socket. This mass would presumably be lightened by pneumatizing the bone—a network of air cells within a mesh of internal bony struts. This supra-mastoid inflation could go up only a short distance without interfering with the line of action of part of the overlying temporal muscle. A similar thickening on the underside, behind the socket, amounted to a very large mastoid



process; but its stabilizing ability was limited. Perhaps mastoids should be even larger, as White (personal communication) has suggested he would have made them.

Seeing that more strength was probably required, I introduced considerable vertical thickening of the rear part of the zygomatic arch (Fig. 5). This allows upward force at the socket to be braced from above, and thus transmitted through the entire arch to the face, and to the rear of the skull as well. This design was modeled on that of the horse skull, which I found has the same discrepancy between the locations of the condyles and braincase walls, as well as a chewing mechanism that closely parallels hominids.

A few other cranial details had to be included for the sake of completeness, but these are of no great significance to evolution or taxonomy. The shape of the brow ridges and the scrap of horizontal forehead just behind them were copied much along gorilla lines. The major muscle insertions on the base of the occipital (neck attachment) were just roughed in according to the hominid pattern. The occipital condyles were located up high against the base of the skull for lack of any reason to draw them down, and also to afford the head the most stable positioning. Petrous bones, basioccipital, and pterygoid wings were all primitive hominid and only roughly indicated.

My construction technique differed between jaw and skull. The jaw was built up in five steps by modeling the parts in Sculpy, which was baked hard after each step. The original plaster jaw was oiled repeatedly to preserve it through these bakings, and it barely survived intact.

In building the skull, I made the jaw sockets of Sculpy, baked them, then glued them to the plaster braincase that was used as a core. The maxilla was made from a half-depth copy of the lower jaw, greatly thickened with more plaster, then carved down. Orbits were made by thickly coating plaster on the outside of a small plastic funnel, which was then removed. The rest of the skull construction involved a slow process of mixing small amounts of hydrocal plaster and adding it on, often incorporating suitably shaped plaster scraps. The final product was scraped and sandpapered until it approached perfection, or until I tired of it.

The skull and jaw were then molded in Silastic (silicone rubber), in two pieces each. Good plaster copies are now being made and distributed.

#### DISCUSSION

I have taken all possible measurements of the finished reconstruction and compared these with the cast of a large male gorilla. Needless to say, *Gigantopithecus* far exceeds the gorilla in every measurement. In three heights and three breadths, the gorilla clustered tightly around 70 percent of this new reconstruction; in three lengths, the discrepancy was less, with the gorilla averaging just over 80 percent.

Wu (1962) gives some measurements of his jaw reconstruction based on

the same male specimen. He adds an admittedly speculative total facial height, and I have calculated the mandible length from his photographs. With this information, we may compare the length, breadth, and height of these two reconstructions with the gorilla.

Measurement	Gorilla (mm)	Krantz (mm)	X-Gor.	Wu (mm)	X-Gor.
Mandibular length	184	208	1.130*	253	1.375*
Bigonial breadth	134	189	1.410*	200	1.493*
Condyle height	124	199	1.524	183	1.476
Facial height	178	255	1.432	320	1.798
Mean of two heights	—	—	1.478*	—	1.637*

Since there are two height measurements from quite different parts of the skull, these are here averaged so their total will be weighted equally with length and breadth. These total ratios, averaging length, breadth, and height (indicated by asterisks) make my reconstruction 1.339 times larger than the male gorilla, and Wu's 1.502 times larger. Whether these figures can be translated into statures or any other gross body dimensions is a matter of speculation.

The volumetric comparisons are found by multiplying the three lineal dimensions together. By this reasoning, my version of *Gigantopithecus* should have weighed 2.3546 times as much as the male gorilla; Wu's version should be 3.3145 times heavier. If the gorilla weighed 180 kg (396 lbs.), then my figures put *Gigantopithecus* at 424 kg (933 lbs.), while Wu's figures give it 597 kg (1,313 lbs.).

These weights need not be taken as definitive estimates because *Gigantopithecus* and gorilla body builds are not necessarily the same. However, if one chooses to use a significantly different weight estimate, then some justification ought to be given for doing so. My own inclination is to use the 424 kg from my reconstruction as the probable male body weight. A relatively more elongated body would be expected in an erect biped as opposed to a knuckle-walker, thus lowering the estimate. On the other hand, the individual used here may be smaller than average, and some of my reconstructions may also be incorrectly small.

And what if Wu's reconstructed size is more accurate?

Most people who have viewed my reconstruction are in no position to compare it with the supposedly living Sasquatch in any knowledgeable way. Those who possess the technical expertise to appreciate the structure of a primate skull have not seen the living animal, and those who claim to have seen a living Sasquatch cannot well interpret their observations in terms of cranial anatomy.



One notable exception is Robert Titmus, a hunter and taxidermist of long experience who claims to have seen several Sasquatches, one of them in full-face and close-up. His opinion, after long study, is that my reconstruction has about the size and general form that he would expect to find in the skulls of the animals he saw. Titmus has one reservation—that the eyes of the animal he claims to have seen were considerably farther apart than my reconstruction would allow. My positioning was based on an expanded gorilla, and certainly could be in error. At the same time, Titmus' observation is from many years ago, and his recollection might not be accurate. There could also be a difference between *Gigantopithecus* and Sasquatch in this part of the anatomy.

#### SUMMARY

The skull reconstructed here is not likely to be far from the actual condition of *Gigantopithecus blacki*. The jaws and teeth are firmly based on actual fossils, and indicate a hominoid primate with affinities more hominid than pongid. Erect posture and a 600-cc brain size are strongly indicated, and these dictate most of the remaining cranial morphology. The total size of the resulting skull would call for a body more than twice as big as that of a male gorilla. That a bipedal hominoid of gigantic size lived in China half a million years ago seems to be well established. Whether an animal of this description is alive today is obviously a different matter, but at least the possibility should be considered open.

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## THE *TZUCHINOKO*, AN UNIDENTIFIED SNAKE FROM JAPAN

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**ABSTRACT:** A curiously short and squat snake has long been reported in the Japanese archipelago. Frequently said to occur in mountainous areas in the center of the archipelago, this reptile has been given as many as 40 different local names. We have retained here the name *tzuchinoko*, which is the most frequently used. Reported descriptions indicate that it is very probably a crotalid snake belonging either to the common species *Agkistrodon halys* (representing abnormal individuals, unusual behavior, or confused interpretations of observations), or to a yet unidentified species.

### INTRODUCTION

The existence of a snake unknown to science has long been reported in Japan. A collection of Buddhistic spiritual tales from the end of the 13th century, and a large, 81-volume Sino-Japanese encyclopedia from the 17th-18th centuries, give the description of a snake known to be short (<1 m) and squat, living on some of the islands of the Japanese archipelago. More recently, its presence has been reported by natives in Korea and New Guinea to Japanese people living in those countries. It may have been eaten by Japanese soldiers in New Guinea during World War II (Yamamoto 1973, 1985).

Among some 40 names used by the Japanese to designate this animal (Yamamoto 1973), we have retained *tzuchinoko*, the most frequently used name. The *tzuchi* is a kind of straw bat whose form is reminiscent of that flying mammal; *tzuchinoko* literally means "the child (or the son) of the straw bat." Most of the other names (such as "rolling-hammer," "snake-bat," "horizontal bat") evoke its characteristic silhouette: a triangular and well-defined head, and a large, squat body with subparallel sides, tapering off abruptly into a thin, short tail which forms the "handle" of the *tzuchi*.

Reported throughout the principal island of Honshu (although less often in the north), as well as the islands of Kyushu and Shikoku, it seems to be absent from the island of Hokkaido and from the small Ryu-kyu and Amami islands (Yamamoto 1973). The more frequent encounters, and also the more recent ones (Yamamoto 1985), have taken place in mountainous regions in the center of the islands. It is not an animal confined to high altitudes; on the contrary, it is most common at lower and middle elevations.

The following reports and descriptions are taken essentially from Yamamoto's works (1973, 1985), and from his letter to us of October 7, 1985. Newspaper articles have also noted the *tzuchinoko* (the weekly *Yomiuri* of June 30, 1973, for example), as has the Japanese *Encyclopedia of the Animals*

*of the World* (Tajima 1973), and a number of general publications ("Reptiles and Batrachians," in *Illustrated Animals*, Gakken, ed.). The *tzuchinoko* has also attracted the attention of Japanese zoologists, whose opinions about it were summarized by Tajima (1973).

### SOME EYEWITNESS REPORTS

Before giving a description of the animal, and discussing its possible systematic position, we think it would be useful to discuss some of the reports, taken from Yamamoto (1973, 1985, and personal communication).

● M. Kyuzo, a mountain dweller living near Hashimoto (a little town south of Osaka), encountered it 30 years ago. On that day, he was working in his field located at the foot of a hill, which was covered with a pine forest and dwarf bamboos. He suddenly heard a loud noise coming down from the hill, and then saw a rolling round and black object, rather similar to a tire of a scooter. The object finished its course without ever rebounding, and disappeared into an irrigation canal, which was dry in that season. Intrigued, Kyuzo went over to the canal. He arrived just in time to see the object unroll, stretch out like a stick, and make a vertical leap and disappear.

● M. Kanelo has seen a *tzuchinoko* three times since 1943, all at the same place (Honshu, right bank of the Hayadashidani, ridge of Mount Bishamon, 973 meters in altitude). He compared it to a weaving loom shuttle, and reported that it descended the wooden slope by rolling. He is one of the few witnesses who consider this animal to be "gentle," and not likely to bite people. The majority of the other witnesses consider the *tzuchinoko* to be aggressive, and they think that its bite is very dangerous, indeed deadly.

● The Tsujimoto couple encountered the reptile near Tojikimi (a mountain village southeast of Osaka) in 1970. Stopping to rest, they sat on a tree stump near what they thought was a broken-off branch. They had scarcely sat down when they saw the "branch" move, and they then realized that it was an extremely thick but short snake. It ascended the path at considerable speed, twisting from the left to the right. They reported a large size, a short tail, and the presence of big, brown-colored scales, marked with dull spots "like those of a boa." They also noted that the body did not seem to represent a circular section, but rather triangular.

● During World War II, M. Foujiwara saw a captive *tzuchinoko* in the region of Tamba, north of Kyoto. The animal had been put in a big cask, in which it lay coiled up in a half circle. Its body thickness was said to be remarkable.

● M. Tokutake, a farmer in the mountainous region in the center of Honshu, is also occasionally a hunter of *mamushi*, *Halys* vipers (*Agkistrodon halys*). He differentiates between the *mamushi* and the *tzuchinoko*, which he claims to have caught twice. The first time, at the end of June, 1969, he



encountered the animal in a grove, between a field and a swamp. He could immediately tell it was not a *mamushi*, he said, because of its squat body, the width of its mouth, and the dimension of the spots on its back. When he approached it, the *tzuchinoko* raised its head, showed its "teeth," and moved its tongue. The animal then reportedly made a leap in the direction of Tokutake, who was able to avoid it. The snake fell in the stubble, nearly 3 meters away. Accustomed to catching snakes, Tokutake captured it with the help of his *sassumata* (a kind of forked stick used by snake hunters). After keeping it captive for some days, Tokutake reportedly killed, grilled, and ate it. It was on the occasion of this "dissection" that our gastronome stated that the tail did not contain vertebrae, and that he found three small half-digested rabbits in the alimentary canal. The specimen measured 54 cm in length, 5 cm in diameter, and had seven dull spots along its back. Tokutake found its flesh more fatty than that of the *mamushi* he was accustomed to.

The second capture took place on August 1, 1971, some 10 km farther north. On that occasion, the *tzuchinoko* was swimming in a spring. Although it again showed aggressiveness, Tokutake tried to capture it. He stated that the skin formed a fold just above the eyes, which were "blinking"; it had what looked like "ears." The animal was "groaning" like a rat, erecting its "ears." Tokutake attributed this behavior to its state of arousal.

● M. Kawano is said to have taken some photos in Yoshino (east of Osaka) in 1968. Unfortunately, he lost the negatives before they could be developed. That same year, in the same region, a *tzuchinoko* was reportedly crushed by a bulldozer, and in 1938, a small girl reportedly brought one to her mother. It had been found dead at the foot of a peach tree; the animal measured 40 cm, had a diameter of a beer bottle (7 to 8 cm), and weighed 3–4 kg.

● More recently, in 1977, three alpinists reportedly observed one at an elevation of 2,200 meters in a swamp forest of the Aka-ishi mountain range (at Honshu, between Nagoya and Tokyo).

● Finally, we wish to point out that Bernard Heuvelmans has recently sent to us a photocopy of a newspaper article showing a photo of an alleged *tzuchinoko* skeleton of about 50 cm, the property of a certain M. Watanabe Masao. Despite bad reproductive quality, it is quite evident that it is a snake skeleton, undoubtedly that of a crotalid. The newspaper in which the photo was published, and Watanabe's address, are unfortunately not known to us at this time.

#### DESCRIPTION

*Morphology.*—The body length varies between 30 and 80 cm, and the width between 7 and 8 cm. The triangular head is distinctly separated from the body by a narrow neck, and is covered by a "rigid skin" (like that of a

crocodile, according to some eyewitnesses) composed of very large scales. The eyes are remarkably big, and movable "eyelids" are said to protect them. Two distinct dimples lie between the nostrils and the eyes (which make one think of the crotalid's thermoreceptive pit organs). The red tongue is reportedly not bifid.

The body has subparallel sides, and a triangular section. It displays large scales, and bears a distinctly doubled mid-dorsal crest. The literal translation of some eyewitnesses' reports (and also of the known consumer, Tokutake) speaks of a "double spinal column." This surprising fact was recently confirmed by Yamamoto (personal communication, October 7, 1985). The posterior part of the body narrows abruptly to end in a small, slender tail, representing less than 15 percent of the animal's total length. Artist's renditions of the *tzuchinoko* based on many reported eyewitness descriptions appear in Fig. 1

The coloration in the dorsal area varies from black to darkened red-brown to iridescent grey (the *tzuchinoko* has the reputation of changing its color). Ten or fewer darker spots are arranged along the body. Ventrally, the venter is a yellowish orange-red, and is covered with large scales, which can be erected. Between the mouth and the eyes there is often reported a well-marked dull band. The ribs are widely separated; there are neither ribs nor vertebrae in the tail. Erectile "ears" are sometimes reported to point outward (probably marking the beginning of moulting).

Finally, many witnesses attribute a disagreeable odor, like that from a chestnut tree flower, to the *tzuchinoko*.

*Ecology and behavior.*—The *tzuchinoko* is generally reported in stubble fields, hillside bushes, wet forests, and hillside swamps. Its altitudinal range is between 1,000 and 2,000 meters. Movement on the ground is by rectilinear locomotion; because of its large ventral scales, it can progress forward in a straight line, without meandering, as many large vipers do (*Bitis*, for example). It swims well, and is able to cross streams. Many people claim that it is able to hang from tree branches by its tail. A very curious way of moving has often been reported: the animal rolls itself into a ball or hoop (as do scaly anteaters and armadillos) and rolls down hills.

The vast majority of Japanese people regard it as being aggressive and venomous. It is believed to stand up erect on its tail before attacking. Others claim that it is able to spit its venom at a distance, cobra-like. It is reported to be very noisy—it "whistles," "hoots," "snores," or "moans" like a rat.

It has been more frequently sighted in fair weather season (from April to October), and when it is overcast ("favorable days to go fishing," according to many reports). It reportedly feeds on small mammals (Yamamoto 1973). No shed skins have ever been reported, and nothing is known about its reproductive biology. Yamamoto (personal communication, October 7, 1985)

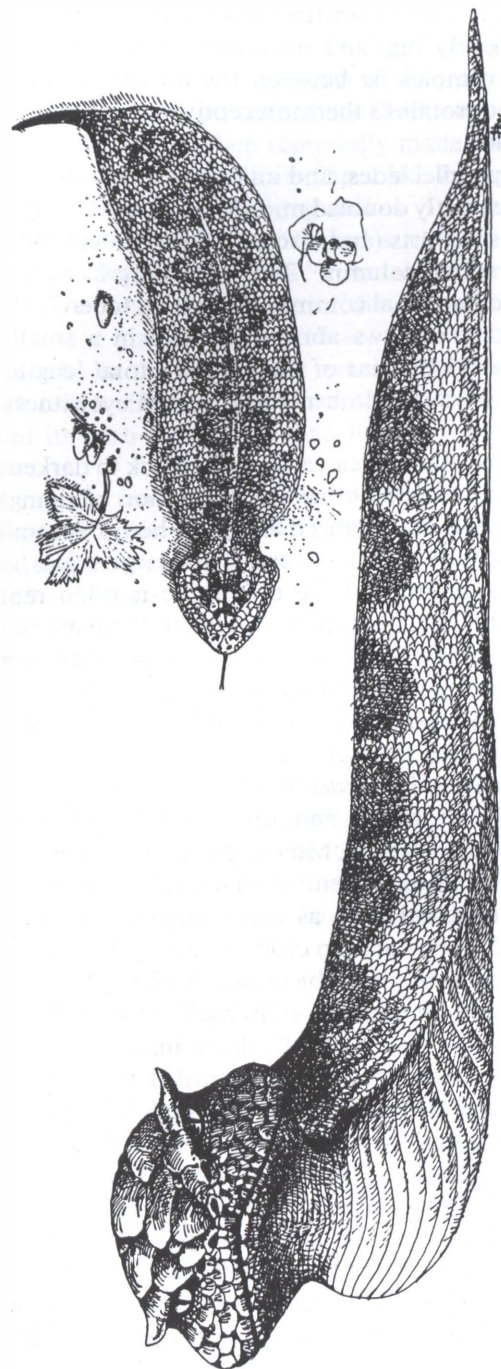


FIG. 1.—Artist's renditions of the *tzuchinoko*, a Japanese snake reported to be different from Halys viper. Based on many eyewitness descriptions.

explains an observation of two individuals “rolled up” as coupling. This more likely would have been two males fighting, perhaps two Halys vipers, *Agkistrodon halys* (Petzold 1974). At rest, the *tzuchinoko* has sometimes been reported coiled-up on itself, in a half or full circle.

#### DISCUSSION

Most Japanese herpetologists do not accept the existence of this supposed reptile, thinking that observations of *Agkistrodon halys* digesting its prey have been falsely interpreted. Sato and Maki especially hold this view (Maki 1931). Others, on the other hand, suggest that *tzuchinoko* reports may contain elements of truth, as it is doubtful that mountain dwellers would systematically confuse it with the Halys viper, and the cultural recognition of *tzuchinoko* has a long history and is widely distributed.

This is far from being an exhaustive literature review of Japanese reptiles. Gans (1949) did an excellent review of the topic. However, from the reports obtained by Yamamoto (1973, 1985), articles by Tajima (1973), and some basic works (Maki 1931, Angel 1950, Grassé 1970, Nakamura and Ueno 1972, Petzold 1974), it is possible to reach some conclusions, following Ueno, curator at the National Museum of Natural History, in Tokyo (Tajima 1973). The characteristics discussed above can be divided into two categories:

1. *Myths, exaggerations, and false interpretations.* The loop position, the presence of “ears,” a prehensile tail, and the spitting of venom, could be based on widespread myths (milk snake, spitting fire, or poisoned breath), attributing to the *tzuchinoko* the characteristics of other species (the venom spitting of the cobras and other *Bitis* or *Crotalus*), or on false interpretations of actual observations (the first stages of moulting, for example, give many snakes the appearance of having ears). The fabulous leaps made by the *tzuchinoko* are certainly exaggerated in terms of height, but a common Montpellier snake is able to jump many meters down a slope. A prehensile tail could well exist in the *tzuchinoko*, as it does in certain other snakes (*Bothrops*, for example). The “double spinal column” may be explained by the presence of a special coloration (*Bitis gabonica*, for example) or carinated scales (as in *Lachesis mutus* or several *Bitis* spp., for example).

2. *Ophidian traits.* Most of the reported traits resemble those of a snake, more precisely a member of the Crotalidae. These are:

- Facial pits (thermoreceptive organs), characteristic of the Crotalidae only.
- Absence of vertebrae in the tail; fusion of the last vertebrae in a style or “terminal body” (Zimmerman and Pope 1948, Angel 1950, Guibe in Grassé 1970).
- A small slender tail which the animal shakes as a warning (Yamamoto 1973, 1985), like in crotalids, but which is noiseless, as in the species



of the primitive Japanese genera *Agkistrodon* and *Trimeresurus* (Nakamura and Ueno 1972).

- The large cephalic scales, also observed in primitive crotalids such as *Agkistrodon* and *Trimeresurus* (Petzhold 1974).
- The diet (small mammals), the rest position (flat spiral), the attack position (S form), and the probable existence of male combat apply to many snakes. The mobile “eyelids” are doubtless nothing more than scales forming folds above the eyes, a very common feature of the Viperidae and Crotalidae (for example, in *Pseudocerastes cornutus*, from Baluchistan, and the southeast Asian *Trimeresurus cornutus*). Finally, all snakes have a bifid tongue, but in some, the base of the tongue is highly colored, while the bifid extremity is less noticeable (as in *Bitis gabonica*, for example).

Other traits, however, recall the Viperidae s.str., especially certain species of *Bitis* (Angel 1950, Petzold 1974):

- Short, squat body, wide head, and well-marked neck (*Bitis* spp., but also *Bothrops alternatus*).
- Triangular section of the body (very conspicuous in *Bitis gabonica*, for example).
- Rectilinear locomotion, without meandering; mostly characteristic of the Viperidae with thicker bodies (*Bitis* spp.), which possess very large ventral scales. Further, these snakes swell their bodies and flatten dorso-ventrally when they are aroused (as reported with the *tzuchinoko*).
- Size of the dorsal scales, undoubtedly exaggerated, but, again, many species of *Bitis* (*gabonica*, *arietans*, *nasicornis*) have remarkably large dorsal scales for snakes of their relatively modest size.
- Raised scales above the eyes, which is observed in *Bitis nasicornis* (Africa), *Pseudocerastes persicus* (Baluchistan), *Cerastes cerastes* (North Africa and Southwest Asia), *Trimeresurus cornutus* (Southeast Asia), and others.

Finally, the noisy nature of the *tzuchinoko* is not surprising. Although “mute,” many snakes nonetheless make some sounds. *Bitis arietans* has a powerful and extended breath, which has given it the nickname of puff adder; *Elaphe taeniura*, the Indochinese and Japanese grass snake, “screams” and “mews” like a cat, and *Vipera russelli* (south and southeast Asia) has a very powerful “whistle” (Angel 1950, Petzold 1974).

#### CONCLUSIONS

Descriptions of the *tzuchinoko* suggest two possibilities, the first being misinterpreted observations of *Agkistrodon halys*, such as individuals swallowing or digesting their prey, abnormal individuals (teratologies?), or in-

dividuals behaving atypically. In the case of such a variable species as *A. halys*, this hypothesis cannot be rejected, particularly since the *tzuchinoko* clearly shows common traits with the *mamushi* (*A. halys*), such as size, marked commissure between the mouth and the eyes, and cryptic coloration and habitat.

*Agkistrodon halys* is indeed a widespread and very polymorphic species. It is a crepuscular animal, encountered at middle and high altitudes (up to 3,200 meters in the Kirghiz mountain range), but it is found usually in mountain forests where its brown-red cryptic coloration provides good camouflage among dead leaves. The back has large, black circular marks (generally 18 to 22, aligned two by two), the neck is well marked, and on the large head, like with most of the primitive members of the Crotalidae, there are large cephalic scales (Maki 1931, Nakamura and Ueno 1972, Petzold 1974).

However, we cannot discard the second possibility; that is, the hypothesis that the *tzuchinoko* may be a new species of *Agkistrodon*. Its striking morphology, the historical span (reported since the 13th century), and the consistency of reports—most of them coming from mountain dwellers who distinguish between the *tzuchinoko* and the *mamushi* (*A. halys*)—argue in favor of this hypothesis. We should also point out that the Japanese commonly are able to distinguish among the five species and subspecies of crotalids living in their archipelago, namely *Agkistrodon halys* (*mamushi*), *Trimeresurus okinavensis* (*himehabu*), *T. elegans* (*sakishimahabu*), *T. flavoviridis tokarensis* (*tokarahabu*) and *T. flavoviridis flavoviridis* (*habu*). (Species of the genus *Trimeresurus* are restricted to the small Ryu-kyu and Amami islands.)

Finally, some *tzuchinoko* traits (body size, scales, triangular section, manner of locomotion) suggest a convergence with certain African Viperidae (*Bitis* spp., for example).

Even in a country as well studied as Japan, the vertebrate fauna can still hold some surprises. In this century, Maki (1930), Okada (1936, 1956), Malnate and Munsterman (1960), Gloyd (1955), and others have discovered and described new varieties, subspecies, and species of reptiles.

#### ACKNOWLEDGMENTS

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## CLONING EXTINCT GENES

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**ABSTRACT:** Genetic material from the remains of both extinct animals and ancient man has been cloned. Such research, presently in its infancy, has shown that DNA can survive for thousands of years essentially unchanged. Further research will yield valuable information regarding the genetic makeup of extinct animals, define their relationship to extant species, and answer concerns regarding the survival of infectious diseases in dead bodies.

## INTRODUCTION

Under certain conditions, cytomorphologic and biochemical integrity of biological material may be preserved for long periods of time. Such material is a valuable source of knowledge about the history and development of extinct and extant organisms. Recently, several investigators have tapped this resource by investigating genetic material from extinct animals and ancient (pre-historic) man (Higuchi *et al.* 1984, Higuchi and Wilson 1984, Johnson, Olson, and Goodman 1985, Pääbo 1985). The results have been a fascinating—although rudimentary—look at ancient genetic and biochemical structure. This paper reviews the highlights of these investigations.

## PALEOBIOCHEMISTRY

A full review of the field of paleobiochemistry is beyond the scope of this paper. However, it is important to recognize that paleo-DNA (deoxyribonucleic acid) studies have their roots in general paleobiochemistry and paleogenetics. The ultimate goal of most of these studies is to establish proper phylogenetic relationships between extinct and extant animal species. Analysis of traces of protein found frequently in “fossilized” specimens yields information regarding structure and function of the proteins; amino acid sequencing of these proteins also provides information on the genetic makeup of extinct animals.

However, there have been problems with this approach. First, since a significant fraction of DNA material in vertebrate cells is not transcribed or expressed (Hickey and Benkel 1986), a study of proteins as a mirror of DNA would, at best, provide only a fraction of the true genetic makeup of such animals. Further, experience has shown this approach has not been particularly successful, in part due to significant post-mortem changes in protein structure (Prager *et al.* 1980). Thus, as DNA purification and cloning technology matured, researchers sought to extract DNA material directly from the remains of extinct animals.



## PALEO-DNA

Under certain conditions, cellular morphology may be preserved for thousands (Lewin 1984) or millions of years (Poinar and Hess 1982). DNA material within such specimens may survive essentially unchanged. DNA has been recovered from the quagga, an extinct equid (100 years B.P.) (Higuchi *et al.* 1984), ancient Egyptian mummies (2,400 years B.P.) (Pääbo 1985), woolly mammoth (10,000 to 53,000 years B.P.) (Johnson, Olson, and Goodman 1985), and insects preserved in amber (approximately 26 million years B.P.) (Higuchi and Wilson 1984).

The quagga (*Equus quagga*) is a zebra-like, ass-like species that became extinct in 1883 (Fig. 1). A salt-preserved skin of a specimen that died 140 years ago, stored in the Museum of Natural History at Mainz, West Germany, was the source of the genetic material isolated and cloned (Higuchi *et al.* 1984). About 0.7 g of muscle and connective tissue adherant to the animal's skin was removed and digested with proteinase K and detergent. The DNA material was purified by phenol extraction and ethanol precipitation, and then fractionated by gel-filtration and electrophoresis. This method yielded about 5 µg of DNA per g of dry tissue (about 1.5% the yield with fresh tissue). Most of the DNA segments were less than 500 base pairs long. About 10 ng of the DNA was cloned using a λ phage cloning vector, which is efficient in cloning small amounts of low molecular weight DNA.

The resultant 25,000 plaques were screened by hybridization with mitochondrial DNA (mtDNA) from the mountain zebra (*Equus zebra*). Two clones were isolated and sequenced, resulting in a total of 229 base pairs available for comparison. There were 12 base substitutions, but only two resulted in amino acid replacement. This pattern (base substitution without amino acid replacement) and degree of sequence difference (approximately 5%) is what is expected when one compares two closely related species. Further analysis allowed for calculation of the quagga-mountain zebra split to have occurred 3–4 million years ago.

Using similar techniques, Pääbo (1985) isolated and sequenced a 3.4-kilobase segment of a 2,400-year-old human DNA. Skin and subcutaneous tissues were collected from a less than one-year-old male infant mummy at the Egyptian Museum in Berlin, German Democratic Republic. The tissue was radiocarbon-dated at  $2,430 \pm 120$  years B.P. Pääbo collected approximately 32 µg of DNA from 1.6 g of dry tissue—a yield of 5% that of fresh tissue. From this, he obtained a segment of 500 base pairs by using a radioactive probe with a member of the *Alu* family of repetitive human sequences. Analysis of the segment showed 77% homology to contemporary DNA—which is comparable to contemporary *Alu* repeats and indicates that there has been no significant post-mortem change over a period of 2,400 years!

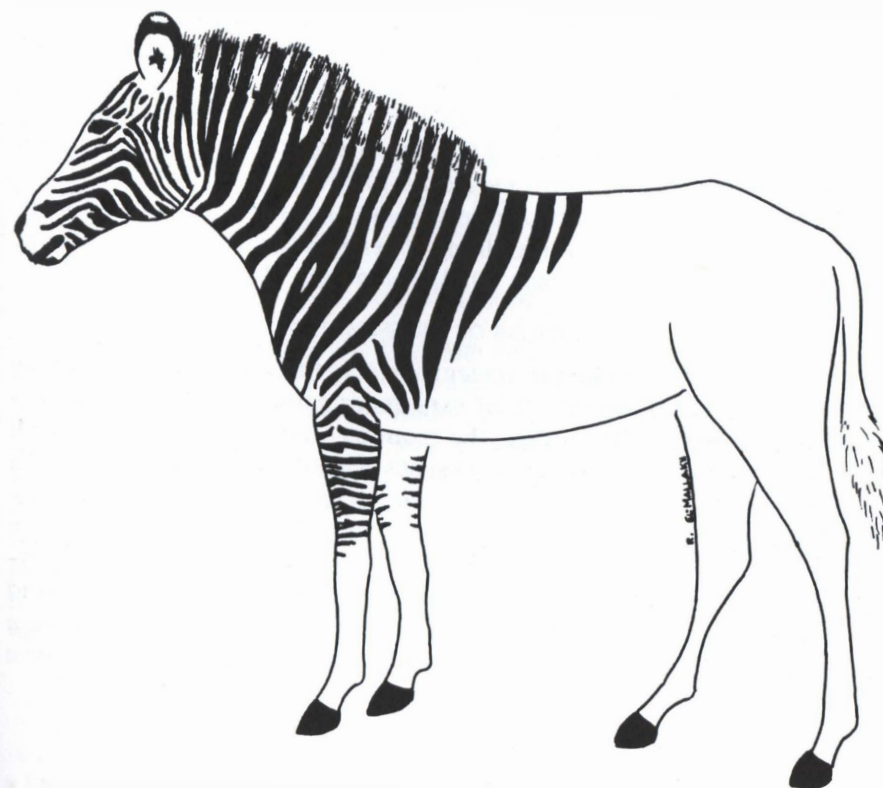


FIG. 1.—The quagga, *Equus quagga*, which is believed to have become extinct in 1883.

Older human DNA has been recovered from a peat swamp in Windover Pond, Brevard County, Florida, U.S.A. (Doran *et al.* 1986). Forty bodies of various ages were discovered in a stratum that was carbon-dated at 7,790–8,290 years B.P. Of these, five individuals had recognizable intracranial brain tissue. Histological analysis revealed remnant cellular structure. DNA was extracted with a total yield of 1 µg/g of brain tissue (1% of yield of fresh brain tissue), of which approximately 0.05% was mtDNA.

Material from two well preserved frozen woolly mammoths, *Mammuthus primigenius*, was the source of the mammoth DNA (Johnson, Olson, and Goodman 1985). Air dried muscle tissue from the Yuribei mammoth, an immature female discovered in 1979 and dated at 10,000 years old, and the Khatanga mammoth, an adult male discovered in 1978 and dated at 53,000 years old, was treated in a similar fashion as described above. The yield was variable but ranged from 8 to 38 µg DNA per g of dried tissue, or 2–12% the expected yield from fresh tissue. DNA fragments ranged from 200 to 3,000 base pairs long. These were cloned in a bacteriophage φX174. Un-

fortunately, there also appeared to be significant amounts of contaminant DNA. When the base composition of the mammoth DNA was compared with DNA extracted from the Asian elephant (*Elephas maximus*), it was found that approximately 25% of the DNA shared a similar base composition (36% G + C) (Johnson, Olson, and Goodman 1985). However, only 2–5% of the DNA was homologous in hybridization studies.

Genetic material from amber-preserved insects has not yet been cloned, but the hardness of DNA over millions of years under these conditions has been shown (Higuchi and Wilson 1984, Poinar and Hess 1982).

#### CONDITIONS OF PRESERVATION

The ultimate limit to cloning ancient DNA is the availability of suitably preserved material. From the brief experience gained, preliminary observations can be made. The quagga, the mummy, and amber insects were all preserved by rapid (days to a few weeks) desiccation, whereas the mammoth was preserved by freezing. DNA yields were relatively similar (in the range of 5–30 µg DNA/g dry tissue), but the mammoth DNA appeared to have a higher proportion of contaminant DNA. This was probably due to intermittent thaw and contamination by microorganisms (Johnson, Olson, and Goodman 1985). Thus, it appears that desiccated specimens may yield more precise sequence data. Further research is needed to determine the prevalence of suitable material.

#### RELATED RESEARCH

In Somalia, on October 26, 1977, a 23-year-old hospital cook acquired a smallpox (variola) infection (Anonymous 1980). He recovered completely, but the event is historic in that there have been no endemic cases of smallpox since. Aside from laboratory-maintained populations of smallpox, the virus has been completely eradicated from the planet. Or has it?

A recent debate has emerged in the medical literature regarding the viability of the smallpox virus (Zuckerman 1984, Lewin 1985, El-Mallakh 1985). There is evidence—both historic and experimental—that smallpox virus may remain viable in dry archaeological material (Razzel 1975, El-Mallakh 1985) or in bodies buried in permafrost (Lewin 1984, 1985). In 1759, at Chelwood, Somerset, England, the body of a man who had died 30 years earlier was exhumed in order to make room for a prominent village resident who had just died. Within several days of the exhumation, all but two people in the village were stricken with smallpox (Dixon 1985). More recently, morphologically intact but nonviable variola virions were isolated from 100-year-old bodies kept in the crypt beneath Spitalfields Church, in the east end of London (Anonymous 1985). Continuing attempts to isolate smallpox from bodies buried in permafrost have, as yet, been unsuccessful (Lewin 1985).

Although there have not been any reported cases of smallpox among archaeologists, anthropologists, or related field workers since the “eradication” of smallpox, the knowledge of long-term survival of dried DNA increases the level of concern (El-Mallakh 1985). Blanket vaccination of workers at risk has been recommended (Zuckerman 1984), but not likely to be carried out (Lewin 1985, El-Mallakh 1985). Further observation and investigation are needed to answer these concerns.

#### CONCLUSIONS

The recent cloning of extinct quagga and mammoth DNA and ancient human DNA raises many questions and opens many possibilities. The observation that DNA preserved under desiccating conditions survives unchanged for thousands of years will allow for more confident study of appropriately preserved specimens. Such research will improve our understanding of the proper phylogenetic relationships of extinct and extant species, which will be of significant interest to zoologists and paleontologists, as well as cryptozoologists. Implications of this research are seen in the medical field, where the long-term survival of viable variola in desiccated bodies could potentially endanger the concept of “eradication” of smallpox.

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## Research Reports

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### THE WILSON NESSIE PHOTO: A SIZE DETERMINATION BASED ON PHYSICAL PRINCIPLES

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**ABSTRACT:** We present a calculation of the dimension of Nessie, as seen in the 1934 Wilson photograph, based on a comparison with the length of adjacent wind waves. The wavelength is estimated from modern results on wind waves and contemporaneous weather information. Quantitative comparison of Nessie and the wavelength scale is effected in a digitized version of the original photograph. Our estimate of the height of the neck above the water level is 1.2 m (4 ft), and other possible (subsurface) dimensions may be estimated proportionally. The object thought to be Nessie in the Wilson photograph is therefore of a dimension which warrants all the interest which it has received.

#### INTRODUCTION

The object which appears in a photograph taken in 1934 by a surgeon, Lt. Col. R. Kenneth Wilson (Fig. 1) has commonly been identified with Nessie, the Loch Ness Monster. Its likeness has appeared on souvenirs and logos, and has entered the public perception as the archetypal form of Nessie. Over the years, however, there has arisen much speculation as to the precise nature of what appears in the photograph.

It is not our intention to contribute to the debate on its authenticity, or to add to the bestiary of alternate explanations proposed: otter, killer whale (orca), diving bird, emerging tree trunk, or swimming elephant. The reader may review the critical history of the Wilson photo by consulting the standard works on the topic (Dinsdale 1961, Burton 1961, Holiday 1969, Costello 1974, Witchell 1976, Power and Johnson 1979, Binns 1984, Campbell 1986, Bauer 1986). The point addressed here, which has long been recognized as a major source of uncertainty, is that of the absence of identifiable features capable of providing a scale for Nessie in the photograph. It has been argued that it matters a great deal whether the object is only a few centimeters long, and thus possibly part of a familiar, small animal, or perhaps a couple of meters long, and thus too large to be so easily explained away.



FIG. 1.—The classic photograph of Nessie, a cropped and enlarged version of the photo taken by Lt. Col. Kenneth Wilson at Loch Ness in April of 1934.

Until the early 1980's, only cropped and enlarged versions of the photo (as seen in Fig. 1) have been available for analysis. Holiday (1969) attempted a calculation of the object's dimensions based on a series of assumptions which he felt were too numerous and shaky to inspire much confidence in the result: about 6 ft (1.8 m) for the neck portion emerging above the water level. Recently, Campbell (1984) discovered an uncropped print at Associated Newspapers (Fig. 2), and he then performed a more direct analysis, which yielded an estimate of 0.7 m for the same dimension.<sup>1</sup> Our analysis is complementary to that of Campbell. It uses the wind waves visible in great number in the uncropped positive of the Wilson photo to deduce a scale for Nessie. The method is similar in principle to that used in scaling Champ in the Mansi photograph of the Lake Champlain Monster (LeBlond 1982).

#### METHOD

A wind-swept sea consists of a confused superposition of waves of a range of lengths and amplitudes, travelling roughly in the direction of the wind (Kinsman 1965). A background of wind waves is thus generally too irregular

<sup>1</sup> More recently, Alastair Boyd found the uncropped version published in the April 28, 1934, issue of *The Illustrated London News*, which he provided to the author—Editor.

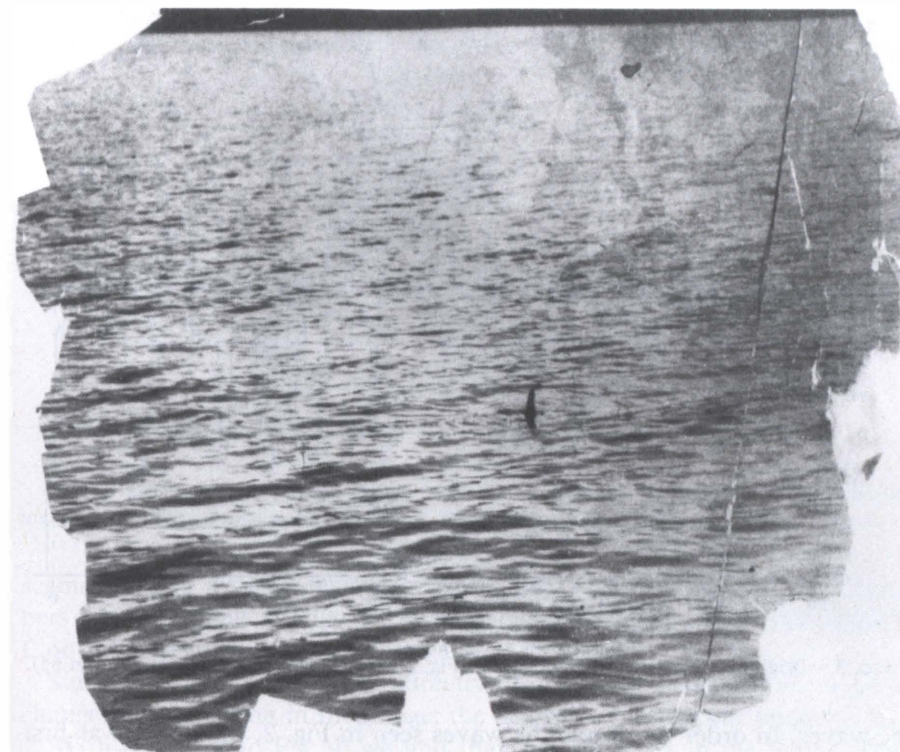


FIG. 2.—The original, uncropped version of the Wilson photo, with opposite shore visible. Note the secondary concentric wave pattern around Nessie; the ellipticity of that pattern has been used by Campbell (1984) to calculate the viewing angle.

to be used as a simple graduated scale against which to measure an object at the sea surface; it may be used as a scale, but only in a rather more subtle way.

The distribution of wave energy among wavelengths (the energy spectrum) has a peak at a wavelength which corresponds closely to the "significant" wavelength which a trained observer would deduce from visual observation. For some angles of view, especially when the waves are seen from the side, as in the Mansi photograph (LeBlond 1982), the significant wavelength is easily determined. This is not the case for the Wilson photo (also known as "the surgeon's photo"), where the wave field is seen nearly head-on. Although we shall attempt a simple scale comparison with what appears to be a single wavelength near Nessie, we shall also use a more objective method based on a quantitative analysis of the wave field.

The distribution of energy among waves of different lengths is usually determined by sampling over a large number of waves, either in time, at a fixed point, or over a broad enough area to average out the irregularities of



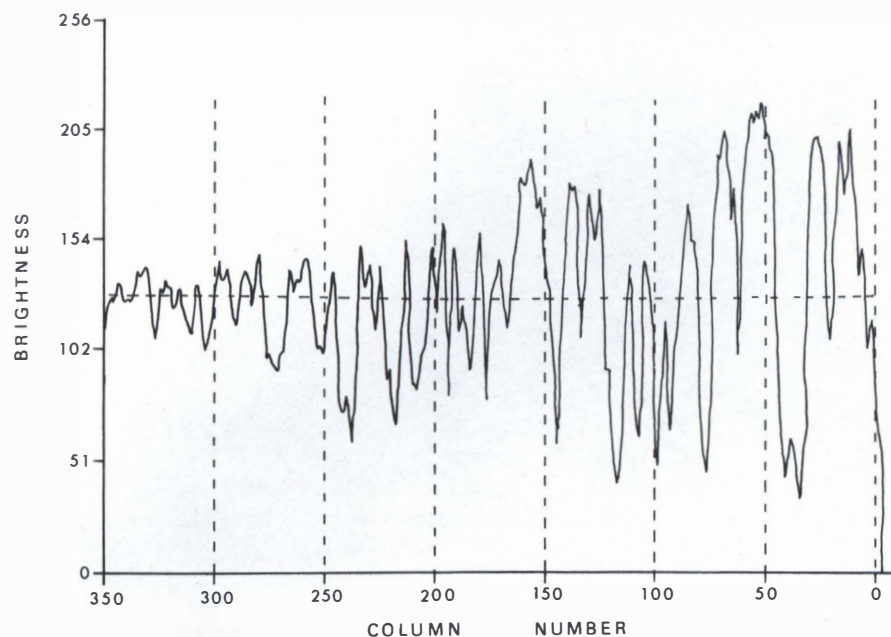


FIG. 3.—Brightness variation along a slice of Fig. 2 near Nessie (which is near column 65).

the waves. In order to sample the waves seen in Fig. 2, the image was first digitized into an array of  $512 \times 512$  pixels of brightness levels ranging from white to black over 256 steps of a gray scale. The brightness level depends on the intensity of reflected sunlight, and hence on wave slope, rather than height. This is an important point in interpreting our results. Brightness levels were sampled along fourteen parallel slices running in the direction of wave propagation, i.e., perpendicular to the wave crests. Fig. 3 shows an example of brightness variation along one of the slices. The numbers of crossings by the brightness curve of its average value were counted over

TABLE 1.—Zero-crossing statistics of brightness fluctuations in the image plane, based on 14 slices such as that shown in Fig. 3.  $n$  is the number of zero crossings in each  $\Delta y = 50$  pixel interval in the direction of wave propagation ( $20^\circ$  from the left of the image vertical),  $y$  is the middle pixel value for each interval from the beginning of the slice, at the bottom of the image,  $y_m = 350$  is the maximum number of pixels in a slice.

$y$	$n$	$y/y_m$	$n(1 - y/y_m)^2$
25	4.6	0.07	4.0
75	6.5	0.21	4.05
125	9.4	0.36	3.9
175	10.3	0.5	2.6
225	10.5	0.64	1.36

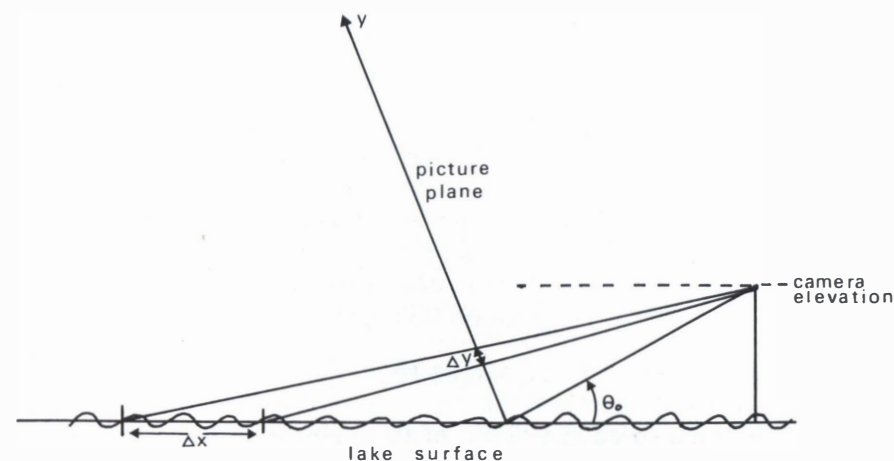


FIG. 4.—Geometrical relations between segments in the plane of the lake surface and the image plane. The viewing angle  $\theta_0$  is calculated from the ellipticity of the pattern in Fig. 2.

segments of 50 pixels along a slice and averaged over all slices. These numbers appear in Table 1. They will be used to determine the average wavelength  $L$  on the basis of two zero-crossings per wavelength.

Since the waves are not seen directly from above, their apparent spacing changes with position in the image: the nearest waves appear longer in Fig. 2. With reference to Fig. 4, we find that a segment  $\Delta x$  in the plane of the lake surface is related to a segment  $\Delta y$  of the picture plane through

$$\Delta x = \frac{\Delta y}{\sin \theta_0 (1 - y/y_m)^2}, \quad (1)$$

where  $y$  is the distance from a reference point on the picture plane,  $y_m$  the maximum value of that distance, and  $\theta_0$  the angle which the picture plane makes with the vertical. We assume that the wave field is statistically homogeneous over the part of the image in which we are interested, so that the compression of wavelengths with distance is entirely due to the angle of view. If there are  $n$  brightness zero-crossings over a chosen picture plane interval  $\Delta y$ , then  $\Delta x = nL/2$ , and from (1) we expect that

$$n(1 - y/y_m)^2 = 2\Delta y/(L \sin \theta_0) \quad (2)$$

should be a constant over a range of values of  $y$ . Table 1 lists average zero-crossing counts over six  $\Delta y = 50$  pixel intervals in the image plane and corresponding estimates of the left-hand side of (2). The angle  $\theta_0$  is taken, as in Campbell (1984), from the apparent ellipticity of the secondary wave patterns concentric to Nessie as  $\theta_0 = 19^\circ$ . The number of pixels per average wavelength  $L$  of the brightness fluctuations may then be evaluated from (2).

The dimensions of Nessie, in terms of numbers of pixels, may thus be expressed in terms of wavelengths.

The final step is, of course, to obtain an estimate of the actual physical dimension of a wavelength. The length of wind waves increases with wind speed, duration, and fetch; empirical results for growing waves are given by Carter (1982). For sufficiently long duration and fetch, waves stop growing and lengthening: the sea is said to be fully developed. Under such conditions, and we shall discuss whether they could have been achieved at the time, the wavelength  $L_m$  corresponding to the peak of the spectrum in deep water (deeper than a wavelength) is given (in meters) by

$$L_m = 0.83 U^2, \quad (3)$$

where  $U$  is the wind speed at a height of 10 m above the water surface, in m/s. Since Loch Ness is long in terms of the fetch required to set up a fully developed sea at the low wind speeds considered (about 20 km), we shall remark only on the possibility of the wind field being duration-limited. From Carter (1982: Appendix A) we find that for duration limited waves

$$L_m = 0.45 D^{0.86} U^{1.14}, \quad (4)$$

where  $D$  is the duration of the wind of speed  $U$  (in hours).

Wind information was obtained from the National Meteorological Library, Bracknell, Berkshire, England, for the presumed date on which the photograph was taken: April 19, 1934, at about 7:00 a.m. (cf. Campbell 1984, for a discussion of that date). On that morning, the nearest Scottish weather stations, at Dalwhinnie (at 56°56'N, 04°14'W) and Aberdeen, respectively reported calm conditions (no wind) and winds from the west-north-west at force 3 (3.5–5.0 m/s). Neither station is representative of conditions at the Loch. The former is located 23 miles southeast of Invermoriston, Loch Ness; the latter, on the shores of the North Sea, is about 90 miles east of Invermoriston. We may, however, take the weak wind conditions measured at the weather stations, together with the surface atmospheric pressure map issued on that morning, as indicative of weather conditions over most of Scotland.

More specific information at Loch Ness comes from the photograph itself. The appearance of the sea surface changes gradually as the wind speed increases in a manner formalized in the Beaufort Scale (Allen 1983). The critical point to note here is that there is not a single breaker visible in Fig. 2. This observation limits the wind speed to force 2 ( $U = 2.5$ – $3.0$  m/s). This is consistent with observations by Monahan (1971) of whitecap coverage, as well as with the general meteorological conditions for that morning.

We now have in hand all the elements necessary to relate the apparent dimensions of Nessie in Fig. 2 to those of the waves, and to obtain an

estimate for the physical wavelength itself in terms of the wind speed. We shall reserve the discussion of possible errors for a later section.

## RESULTS

Let us first estimate the wavelength of the spectral peak from observed wind conditions. We shall assume that the wind was blowing from the north end of the Loch, as inferred from the photograph and from the general weather conditions, at a speed appropriate to Beaufort force 2:  $U = 3.0$  m/s. The presumed location of the photograph, 2–3 miles on the Inverness side of Invermoriston, gives an upwind fetch of about 20 km. From Carter (1982), a duration of 6 hr is required for the full development of a wind sea for that fetch and wind speed. From (3),  $L_m = 7.5$  m under those conditions. It is quite conceivable, however, and perhaps more likely in view of the frequent diurnal modulation of valley winds, that the wind might not have been blowing for 6 hr or more. Wilson's recollections, as quoted by Campbell (1984), are uninformative in this respect. For a shorter duration,  $D = 3$  hr, (4) yields  $L_m = 4.0$  m.

The other point to recall is that what is observed in Fig. 2 is a distribution of wave slopes, not of wave amplitudes. The results just quoted for  $L_m$  are for wave amplitudes; the slope spectrum is obtained from the displacement spectrum by dividing by the square of the wavelength; its peak occurs at shorter wavelengths. The JONSWAP spectrum appropriate for growing seas is quite steep, and a modicum of algebra shows that the length  $L$  of the peak of the slope spectrum is very near  $L_m$ :  $L = 0.95 L_m$ . The difference is too small to be of concern to us in the present context.

In view of the above, and on the basis of the first author's personal experience with a similar sea state on Loch Ness, we shall adopt  $L = 4.0$  m as the wavelength of the brightness fluctuations in Fig. 2. The reader will, of course, be aware of the subjective aspect of this choice, and may wish to scale the results up to  $L = 7.5$  m, which would correspond to the absolute upper limit for the wind force and sea development.

It is useful at first to short-circuit the painstaking pixel counting process leading to (2), and to perform a direct—if rather subjective—visual comparison of Nessie with nearest wave. In Fig. 2, we judge that Nessie occupies about 70% of the vertical distance in the image plane between the nearest dark bands on either side of it. A correction by the factor  $(1/\cos[20^\circ] = 1.06)$  to take into account the angle of propagation of the waves brings that ratio up to 74%. Taking the viewing angle into account, as shown in Fig. 5, we have  $h/x = \tan(19^\circ)$ . With  $x = 0.74 L$ ,  $h = 1.06$  m (3.5 ft). This estimate is surprisingly close to that originally given by Wilson (Anonymous 1934), who reported: "I saw a long neck, about three feet out of the water, and a head which looked small."

Results of the more objective wavelength estimation based on zero-cross-



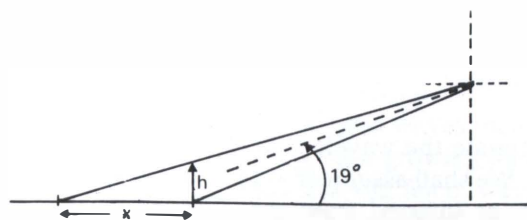


FIG. 5.—The geometry pertinent to calculation of Nessie dimension from comparison with wavelength.

ings are included in Table 1. The last column gives estimates of the right-hand side of (2) for a sequence of bands of 50 pixels in the image plane; these numbers should all be the same for purely geometrical distortion. Because the fourth and fifth bands correspond to the top of the image, where digitization leads to serious loss of resolution, we are satisfied that the wave field seen in the first three bands is homogeneous, and that the value 4.0 may be used for the right-hand side of (2). We then calculate

$$L = \frac{y}{2 \sin(19^\circ)} = 77 \text{ pixels}$$

in the plane of the lake surface. In the image plane, a wavelength then covers  $L \sin(19^\circ) = 25$  pixels. The dimensions of Nessie in terms of pixels in the image plane are shown in Fig. 6. Instead of 70% of a wavelength, as in the above "simple" analysis, we now have  $22/25 = 88\%$  (there is no need to apply the  $1/\cos[20^\circ]$  correction since the wavelength is already calculated in the direction of wave motion). Using the same viewing angle as above, the geometry of Fig. 5 now gives  $h = 0.88 L \tan(19^\circ) = 1.21 \text{ m}$  (4 ft).

#### DISCUSSION

Our analysis yields a value of 1.2 m (4 ft) for the height of Nessie's neck as it appears in the photograph. Other possible (subsurface) dimensions may be estimated in proportion from Fig. 6. We have discussed and presented all assumptions made in obtaining this result, so that the reader may be aware of its limitations. The most important assumption concerns the duration of the wind before the photograph was taken. Shorter (1 hr) or longer (6 hr) assumed durations change the wavelength of wind waves up or down by a factor of two, with a similar effect on Nessie's size estimate.

The results are too inconclusive to draw specific biological conclusions, although perhaps the bird hypothesis can be discarded. From a general point of view, we find that the object seen in Fig. 2 is of a dimension which certainly warrants all the interest which it has received and the mystery which surrounds it.

We are grateful to Tom Lee, of the University of British Columbia Com-

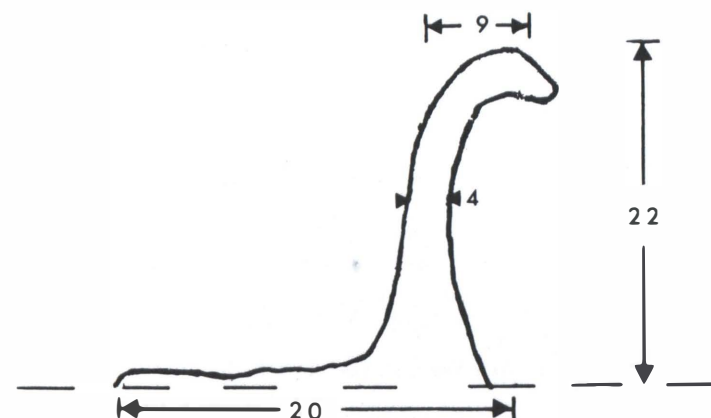


FIG. 6.—Pixel dimensions of Nessie in the image plane, based on a  $512 \times 512$  digitization of Fig. 2. Drawing not necessarily accurate.

puting Centre, for his assistance in image digitization, to Paul Nowlan and Andrew Thomas, of the Satellite Oceanography Laboratory, for their advice in image manipulation, to R. M. Mason, of the National Meteorological Library, United Kingdom, for providing weather information, to J. Richard Greenwell for his bibliographic assistance, and to Alastair Boyd for pointing out to us Wilson's original estimate of Nessie's dimensions.

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## Field Reports

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### NEW SIGNS OF SASQUATCH ACTIVITY IN THE BLUE MOUNTAINS OF WASHINGTON STATE

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#### INTRODUCTION

On the night of April 9, 1987, Washington State University anthropologist Grover Krantz received a telephone call from Paul Freeman, an active Sasquatch investigator, concerning new possible Sasquatch findings that he had discovered earlier that day. He stated that, two days earlier, he had heard a second-hand account of a family of campers that had been disturbed during the night by a series of loud screams and the snapping of branches upslope from where they were parked. They had reportedly become so unnerved that they had fled the area. This incident occurred in the Blue Mountains of Washington State, less than an hour's drive from the city of Walla Walla.

Upon hearing of this incident, Freeman stated that he had driven to the site, clambered up a 12-foot embankment, and discovered a well-worn trail that paralleled a ridge for several miles in either direction. The elevation of the trail ranged from 4,000 to 4,200 feet above sea level. He informed Krantz that, upon attaining the trail, he had immediately discovered tracks that he interpreted to have been made by a Sasquatch. The tracks were evident for nearly three quarters of a mile, and appeared to have been made by up to four individuals. Two tracks had a maximum length of 14.5 inches, as measured from the most posterior position on the heel to the most anterior





FIG. 1.—Three of the 17-inch tracks, only one clearly visible in foreground, showing splayed five digits. Heel width was 6 inches. Observed initially by Paul Freeman upon his arrival at site on April 9, 1987. (Paul Freeman.)

position of the longest digit. Another track was approximately 17 inches long (Fig. 1). A fourth track had a length of about 11 inches. Only the three larger tracks had been deposited with sufficient detail to be cast, and Freeman had made one from each of these. More interestingly, he also reported that he had discovered snapped trees, a possible “bed,” and what he thought might be Sasquatch hairs.

At 12:30 p.m. on April 11, Krantz, accompanied by myself, Mark Mansperger, John Cardinal (two other graduate students from the Washington State University Anthropology Department) and Markku Niskanen (an undergraduate), joined Paul Freeman in Walla Walla. He took us to the site where he had originally discovered the tracks. While en route, he informed us that, on the preceding day, he had found more tracks nearly 5 miles farther along the trail that appeared to have been made by the individual with the 17-inch-long foot, as well as more broken trees and hair samples. He had made two more casts.

While at the original site, we were joined by another Sasquatch investigator, Greg May.



FIG. 2.—A right footprint of the 17-inch track individual. The heel is extended in this photo due to dragging. (Lonnie Somer.)

#### NARRATIVE DESCRIPTION

Although numerous tracks were evident at the original site, most were either partially or completely obliterated by the time of our arrival. There were three causes for their disturbance: 1) In order to obtain impressions of the tracks while they were still fresh, it was necessary for Freeman to pour plaster into the depressions. The removal of such a cast invariably destroys the original track; 2) it had snowed heavily the previous night, there had been some melting, and it was commencing to snow again upon our arrival. This caused a certain amount of fine detail to be lost; 3) Freeman was not the only person to have been informed about the plight of the campers four nights previously. Word had spread, and others had preceded us to the site, often walking along the trail indiscriminantly and superimposing their footprints upon the original tracks.

Nevertheless, we were still able to find a few nearly intact prints, mostly just off of the trail (Fig. 2). One of these was of scrape marks in the soil consisting of five digits of equal length; we initially assumed they were made by a hand. This was cast by Krantz. Subsequent examination of the resultant plaster impression revealed that it matched the digits of the 17-inch-long





FIG. 3.—Grover Krantz with snapped-off pine tree top, demonstrating how he thought this had been done. (Lonnie Somer.)

footprint cast very closely, so it was decided that the dig marks were actually made by that individual's toes. However, a very clear handprint was discovered approximately 10 feet downhill of the trail on a 30 degree slope. This was also cast by Krantz. In addition, Greg May made casts of three other tracks that were still intact enough to be recognizable.

Due to the increasingly deteriorating weather, and the time that it had taken to examine the finds and make the plaster replications, only about 200 yards of the trail were covered by our group. Within this limited area, a dozen small pine trees were located along the trail that had been snapped in half from 2 to 5 feet above the ground. Their diameters ranged from  $\frac{3}{4}$  to nearly 2 inches. At least two appeared to have been twisted off. Some of the broken tops had been left hanging by their bark to the trunks (Fig. 3). Others were lying upon the ground. The needles of the broken trees were still green, and the sap was still oozing from the trunks, indicating the

freshness of the breaks. On a few of the trees, what appeared to be hair was found next to the breaks where it had gotten caught in the sticky resin.

Approximately 150 yards along the trail from where we had initially arrived, and nearly 10 feet downslope, was a slightly depressed and level area measuring 3 feet by 6 feet. Small branches containing numerous needles had been freshly broken off of a nearby pine tree, from a height of 3 feet to about 7 feet, and had been placed along the downhill edge of the depression. More presumed hair was also found throughout the depression; this matched the previously found samples in a superficial inspection of color and thickness. One could speculate that this spot was used as a nest, although this is far from certain.

More presumed hair was found about 6 feet above the ground on the trunk of a very large pine tree. Ranging from 2 to  $3\frac{1}{2}$  feet above this were numerous scratch marks on the side of the trunk facing the trail. They appeared to have been made by somewhat ragged fingernails, since each scratch was of unequal depth and much too wide to have been made by a claw.

A log depression was found outlined in the soil a few feet from the trail. From the appearance of the soil, the log that had formerly lain there had been recently moved. The log in question was found several feet downslope. It was 12 feet long and approximately 1 foot in diameter, with a large root cluster as its base. There were no drag marks along the ground, although an intervening log did have recent scrape marks along its surface.

Also found were areas where the earth had been scooped up, possibly to obtain the roots and bulbs of various small plants, such as blue-eyed grass and yellow buttercups. Some of these roots and bulbs were found with their underground sections removed, and only the stems and leaves lying near the disturbed soil.

## RESULTS

A laboratory inspection of the casts was conducted during the week following our return. Of the three recognizably different individuals, a cast of the foot of one clearly matches impressions that had formerly been left in the same vicinity by an individual that has been dubbed "Dermals." Its maximal length is 14.5 inches, and it has a heel width of 4.5 inches. The base of the digits are sloped, with digit I being the farthest removed from the heel. Another right foot cast closely resembles another, previously known individual, who has been ignominiously named "Stud." This particular cast has an excellent series of dermal ridges across the midfoot, and some on its heel. Its dimensions almost match that of "Dermals," except that the base of its digits form a nearly straight line. The 17-inch-long casts have a heel width of 6 inches, and appear to represent an individual previously unknown



to this area. A few of the other footprint casts also display dermal ridges, although to a lesser extent and of poorer quality. The handprint cast possesses four clear fingertips, including one on which dermal ridges are evident.

The presumed hair samples that were brought back range in color from light red to grey. They vary from less than two inches to nearly a foot in length, and are extremely fine. Samples have been sent to various laboratories for microscopic and biochemical analyses. In addition, I am planning to conduct a comparative microscopic analysis using human and ape hairs. The one obvious problem with this approach is that one can demonstrate that a hair was or was not derived from a known species, but one cannot prove that it came from a Sasquatch, since there are no undoubted Sasquatch hair samples to use as controls.

As to why such an unprecedented display of possible Sasquatch behavior (the broken trees, the scratched tree) was exhibited, can only be a matter of unfounded speculation. Freeman felt that the new individual with the 17-inch track may have been following the others, but he was not certain. Perhaps the broken trees and scratch marks represent the territorial signs of a visually oriented animal trying to establish its dominance. Only additional field research on the Sasquatch question can shed light on this question.

#### FUTURE PLANS

We intend to continue searching, as time permits, throughout the Blue Mountains range for more Sasquatch signs, and to continue to document any new finds that we may uncover.

## LCPI WORK AT LAKE CHAMPLAIN, 1987

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### INTRODUCTION

The year 1987 marked a new era in underwater exploration technology for the Lake Champlain Phenomena Investigation (LCPI) in its search for "Champ," the 15-25-foot-long animals said to inhabit Lake Champlain. The more conventional forms of searching for these supposed animals—deploying surface surveillance with cameras and binoculars/telescopes, scuba searches, and night-scope surveillance—were again used by LCPI during 1987. However, more sophisticated equipment, including side-scan sonar, an underwater robot called Remotely Operated Vehicle (ROV), and a video digitizer monitoring system were also employed at the 400-foot-deep and 109-mile-long Lake Champlain, located in Vermont, New York, and Quebec. This report serves as an update on LCPI's ongoing fieldwork; it also reviews the recorded 1987 Champ sightings.

The 30 days of LCPI fieldwork during 1987 were primarily directed at: (1) side-scan sonar surveys searching for a possible Champ carcass lying on the lake bottom, or a mid-water Champ target; (2) eyeballing any sonar-discovered bottom targets indicating a possible Champ carcass, and searching underwater dropoffs using the ROV; (3) conducting the first field tests of an experimental video digitizer monitoring system designed to observe the lake's surface and to activate a video recorder when unusual surface movement is detected; (4) daylight surface surveillance with cameras and binoculars/telescope from boat and shore-based sites; (5) underwater searches using scuba divers; (6) further documentation, analysis, and publication of reported Champ sightings; (7) encouraging residents and visitors at Lake Champlain to carry cameras to possibly get photographic documentation on Champ; and (8) providing assistance to serious individuals and groups involved in Champ-related research and fieldwork.

### NARRATIVE DESCRIPTION

The LCPI conducted 30 days of fieldwork at Lake Champlain during 1987. Donated equipment came from Klein Associates, Inc., Salem, New Hampshire; Kaselaan & D'Angelo Associates, Inc., Haddon Heights, New Jersey; Vermont Dive Charter, Jericho, Vermont; and Susan Schmidt, New York City. Klein Associates, Inc. provided a Klein 500 kHz and 100 kHz side-scan sonar. Garry Kozak, Manager, Field Operations, Klein Associates, Inc.,

acted as our primary side-scan sonar operator. Kaselaan & D'Angelo Associates, Inc. provided a Minirover Mk II ROV, equipped with a VHS video camera and a manipulator arm for object retrieval. The ROV has 500 feet of umbilical cable, giving us an ability to reach any depth in the lake (Fig. 1). Vincent Capone, Division Manager, Marine & Aquatic Sciences, Kaselaan & D'Angelo Associates, Inc., and Chip D'Angelo, President of Kaselaan & D'Angelo Associates, Inc., were present as ROV operators and also as part of the LCPI scuba team. Captain Rafael Veve of Vermont Dive Charter donated the use of his 28-foot-long vessel, the *Neptune Star*. John Becker donated an ImageWise video digitizer, an Atari 1040ST computer, and a Panasonic surveillance camera. Susan Schmidt donated night-scope gear for low visibility surveillance. M. Patram Meaney and Joseph W. Zarzynski donated various camera and video gear, including a Magnavox surveillance camera and monitor, and a Panasonic video recorder for the video digitizer monitoring system.

Zarzynski participated in all 30 days of LCPI fieldwork. Others participated on the following date(s): M. Patram Meaney (April 11; June 25–30; July 1–3, 18; August 5–14), Vincent Capone (June 26–28; August 7–9), Chip D'Angelo (August 7–9), Garry Kozak (June 26–28; August 7–9), Donald Mayland (June 26–28; July 2; August 7–9), Russell Bellico (July 15, 26; August 7, 12, 18), John Becker and Ted Straiton (August 1–4; October 3), Bruce Hallenbeck (August 1–3), Captain Rafael Veve (June 26–28; August 8–9), Captain Dan Couture (August 7, 18), Jim Rock (August 7), Paxti Pastor (August 8–9), Willie Veve (August 9), Jack Sullivan (August 18), David Pitkin, Ken Bartowski, and Karen Lichter (October 3), and various people during a public "Champ watch" aboard the vessel *The Spirit of Ethan Allen* (July 27).

Special thanks go to Richard Smith and Eugene Lepeschkin for bringing to my attention some previously unrecorded Champ sightings, and to George Zug, Clive Cussler, Robin Holmes, and Martin Klein for their advice in proper use of side-scan sonar and ROV technology.

The 1987 LCPI fieldwork was undoubtedly the most technologically advanced of any previous LCPI directed expedition. Fieldwork consisted of: daytime surface surveillance from boat and shore using 35mm cameras, a Super 8mm camera, telephoto lenses, camera tripods, binoculars/telescope, surveillance camera/monitor, video recorder, and computer with digitizer; nocturnal surface surveillance using a hand-held night-scope; scuba monitoring; scuba diver underwater photography and videography; side-scan sonar; and use of the ROV.

Lake surface surveillance was conducted primarily from Kimball Dock Pier, Vermont. Numerous surface surveillance sessions were carried out from boat. Side-scan sonar sweeps and underwater videography using the ROV were conducted from the *Neptune Star*.

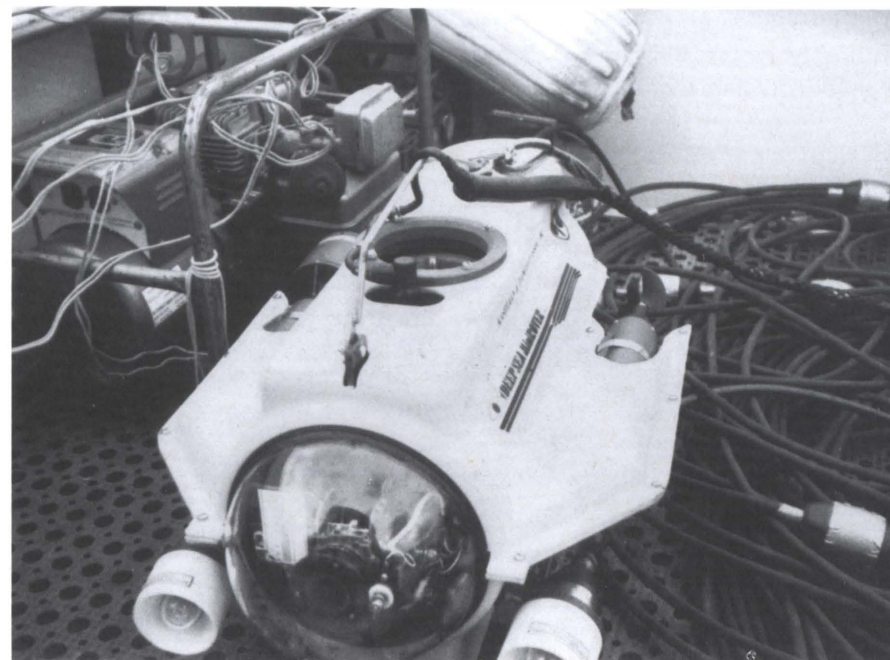


FIG. 1.—The underwater robot ROV (Remotely Operated Vehicle) deployed at Lake Champlain in 1987. It is equipped with a VHS video camera, a manipulator arm for object retrieval, and a 500-foot-long umbilical cable capable of reaching any depth in the lake.

The dates of the fieldwork were: April 11; June 25–30; July 1–3, 15, 18, 26–27; August 1–14, 18; October 3. Daytime surface surveillance from shore sites or boat using cameras and binoculars/telescope was conducted during all 30 days; nocturnal surface surveillance from a shore site using a night-scope was done during one night; boat-deployed side-scan sonar was used during six days; the ROV deployed from a boat was used during five days; and 17 scuba dives were conducted, nine with a diver using a Nikonos underwater camera, two with a diver using an underwater video camera, and three with a diver/divers working with the ROV.

The 17 scuba dives were made as part of an underwater reconnaissance survey, to inspect the ROV, or to examine side-scan sonar targets. The dates of these scuba dives, the divers involved, and the type of dive were: June 25 (Meaney, Zarzynski; search dive), June 26 (Mayland; inspect ROV), July 1 (Meaney, Zarzynski; search dive), July 2 (two dives: Mayland, Meaney, Zarzynski; search dives), July 15 (two dives: Bellico, Zarzynski; search dives), July 26 (two dives: Bellico, Zarzynski; search dives), August 2 (Zarzynski; search dive), August 8 (Capone, Pastor; examine side-scan sonar targets/inspect ROV), August 9 (D'Angelo, Pastor; examine side-scan sonar targets/



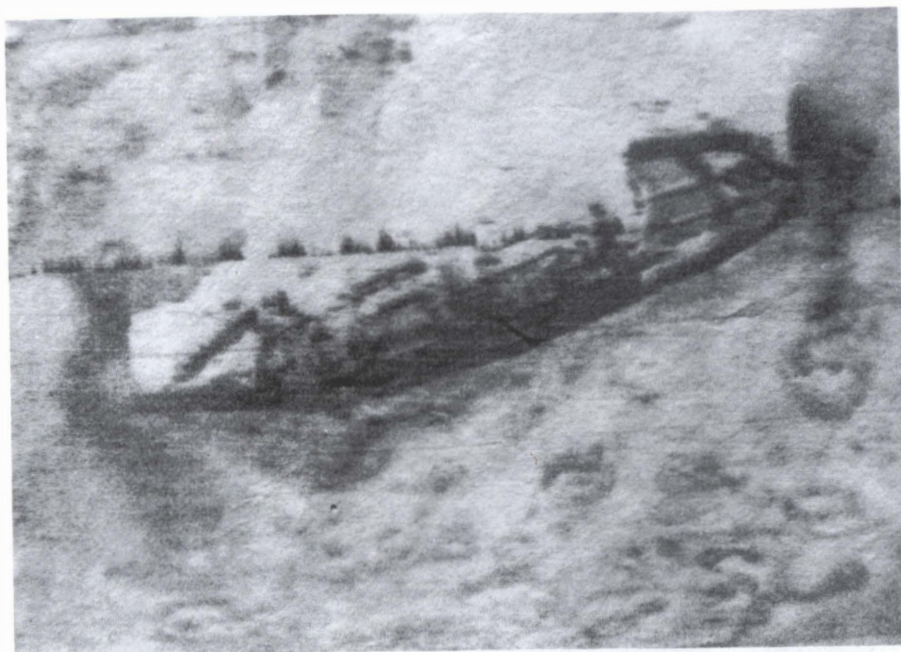


FIG. 2.—Underwater wreck, probably of the tugboat *William McAllister*, discovered in 150 feet of water during side-scan sonar survey on August 7, 1987.

inspect ROV), August 12 (two dives: Bellico, Zarzynski; search dives), August 13 (Meaney, Zarzynski; search dive), and August 18 (two dives: Bellico, Couture, Sullivan, Zarzynski; search dives).

#### RESULTS

LCPI personnel reported no visual sightings of Champ during the 1987 fieldwork. However, on June 28, the LCPI team (during Project Champ Carcass, a six-day side-scan sonar/ROV Champ carcass search) observed two deer in the lake, swimming from Schuyler Island west toward the New York shore. In my 14 years of work at Lake Champlain, this is the first time I have observed deer swimming in the lake.

On August 7, the LCPI team, while searching for a Champ carcass, discovered a shipwreck lying in approximately 150 feet of water during a side-scan sonar survey off Schuyler Island (Fig. 2). Garry Kozak, our side-scan sonar operator, interpreted the sonar target as the wreck of a tugboat. Follow-up research has uncovered that the wreck is probably the tugboat *William McAllister* that went down in the lake in November, 1963. The appropriate authorities have been notified of the discovery of its location. Also, our side-

scan sonar survey noted several deep water targets lying on the lake bottom that deserve investigation by the ROV. Although probably geological features or sunken trees, the targets correspond to the size dimensions attributed to Champ.

The LCPI was able to uncover two more 1986 eyewitness sightings not included in last year's report (Joseph W. Zarzynski, 1986, LCPI Work at Lake Champlain, 1986, *Cryptozoology*, Vol. 5: 77–80). This brings the number of 1986 Champ sightings to 13. These two previously unrecorded sightings are: 1) August 31, 1986: David Christensen; from boat located between Willsboro Point, New York, and Four Brothers Islands; 5:30 p.m.; clear weather, very calm lake surface; "it looked like a snake standing on the water"; a 5-foot long neck, greyish-black in color; 2) June 3, 1986: Barbara Fortune; Clay Point area, near Colchester, Vermont; about 5:00 p.m.; calm water surface; sunny; 3 to 4 dark humps undulating, some splashing.

The seven 1987 Champ sightings uncovered to date by LCPI are listed below in chronological order (Fig. 3):

- April 25, 1987: Lennie French; off Barber Point, near Westport, New York; 3:00 p.m.; lake was mirror-calm; two humps about 1 1/2 to 2 feet high; greyish green-brown in color.
- June 23, 1987: Cedric E. Haskins; Keeler's Bay, South Hero, Vermont; 5:00 p.m.; range, about 200 feet; "very calm, bay was like glass"; head/neck seen; "between black and very dark brown" in color; head was "angular and sharp-snouted."
- June 17, 1987: Dwight Burnham and wife; near Crown Point bridge; 6:15 a.m.; observed 10–15-foot-long dark object on the surface.
- July 4, 1987: Kathy Allmon Goodrich and husband; Basin Harbor, Vermont; no time or weather information provided; while canoeing; animal with two distinct humps; a third hump just breaking water; about 40–50 feet in front of canoe; dark in color.
- July 26, 1987: Alison, James, Lauren, and Nicky Murphy with Richard Cowperthwait; south of Kimball Dock, Vermont; 8:00 p.m.; range, 1/2 mile; calm lake surface; "a dark, sinuous object about 18 feet in length moving through the water"; 3–4 humps (Alison and James Murphy claim they saw a head, described as being flat and dark).
- September 12, 1987: Marilynne and Brent Holden; Treadwell Bay, New York; 4:00 p.m.; range, 24 feet; lake "almost calm, some slight chop"; described as "a young specimen of the species"; "no distinguishable head"; black, dull (not shiny) in color; only 3 feet of animal visible; 2 humps; several photographs taken by Marilynne Holden using a Canon AE1 camera with 85–205mm lens.
- September 13, 1987: Marilynne and Brent Holden and Irene and Bob

Sinclair; Treadwell Bay, New York; 4:15 p.m.; believed they saw the same animal as on September 12; proposed the animal may be feeding in the shallows at end of their dock.

#### FUTURE PLANS

The LCPI will continue its fieldwork at Lake Champlain during 1988, with an increased emphasis on underwater searching. This year's participants have already committed themselves to a continuation of Project Champ Carcass and other LCPI activities. Next year's fieldwork will include use of the ROV examining deepwater rocky dropoffs—underwater terrain that might conceivably snag and hold a Champ carcass. Further side-scan sonar surveying will be conducted in search of targets that might be of a Champ carcass. Those targets will be noted by Loran C, and then eyeballed using scuba divers or the ROV. Several such targets from the 1987 side-scan sonar work will be examined by divers or the ROV. John Becker will continue experimenting with a video digitizer monitoring system. Next year will also see an underwater surveillance camera used to test this system. Vincent Capone, working with LCPI data, plans to complete a computer study on known Champ sighting events, which could assist cryptozoologists in future fieldwork strategy.

This increased effort in underwater searching could ultimately lead to the definitive evidence required for Champ—a carcass. Although LCPI does not advocate the harming or killing of these unknown animals, it believes that, by securing a Champ carcass, a major step would be taken leading to Champ's formal recognition, which would ensure protection for the species.

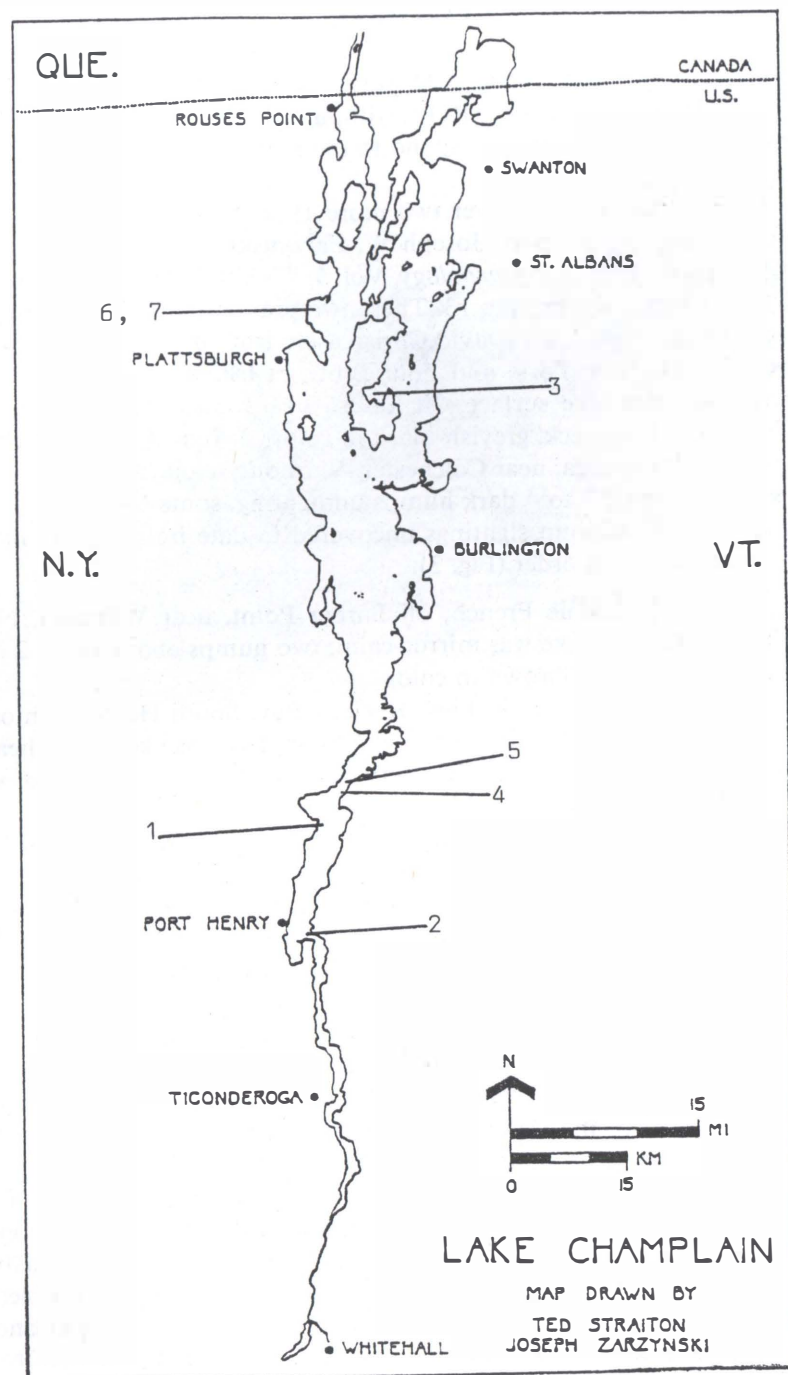


FIG. 3.—Map of Lake Champlain, with numbers indicating the locations of the 7 eyewitness sightings logged by LCPI during 1987.



# OBSERVATIONS OF TWO LINES OF SASQUATCH TRACKS IN OREGON

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## INTRODUCTION

In November, 1986, the author spent several days examining, measuring, making plaster casts, and photographing presumed Sasquatch (Bigfoot) tracks. The location was in the Clackamas River drainage of the Cascade Range in northwest Oregon. Earlier fieldwork has appeared in this journal previously (James A. Hewkin, 1986, Investigating Sasquatch Evidence in the Pacific Northwest, *Cryptozoology*, Vol. 5: 27-37).

The tracks were reported by a boy who took a crude plaster cast (21 inches) of a presumed Sasquatch print to Jack Sullivan, who then relayed the information to me. Sullivan investigated Sasquatch reports with me in this general area in 1976-77, and we were interested in an opportunity to investigate new evidence.

## NARRATIVE DESCRIPTION

On November 12, 1986, Jack Sullivan and I visited a site in the Cascade Range foothills where a boy showed us numerous presumed Sasquatch tracks. Upon close scrutiny, it was evident there were two trails of tracks, both indicating widely spread, flexible toe action.

At the outset, I was skeptical of the validity of the prints, mainly because of their close proximity to human habitation and activity. The two trails of tracks started abruptly and ended abruptly. The tracks began about 300 yards from the family cabin where the boy lives with his parents and 3 brothers. They were logging the timber off a 1,700 acre tract, and considerable equipment was on the premises. The chassis of an old school bus and a large truck loaded with cord wood were in close proximity to the tracks. The grounds adjacent to the cabin are occupied by a menagerie of animals, including several chained dogs, some goats, chickens, ducks, geese, and a horse.

The most prominent feature of this mountainous foothill vicinity is vast tracts of second growth Douglas fir interspersed with woodland farms. The elevation is about 2,300 feet; however, within a few miles, the elevation exceeds 4,000 feet. It is rugged terrain dissected with numerous logging roads, and logging activity is in progress year-round.

In conversation with the boy and his father, it was learned that considerable disturbance had been going on among the farm animals for several nights prior to the discovery of the tracks. The chained dogs reportedly acted

TABLE 1.—Lengths of measurable strides in two lines of presumed Sasquatch tracks found in the Clackamas River Drainage of N.W. Oregon Cascade Range, November 7, 1986.

Upper track line strides (inches)	Terrain	Remarks	Lower track line strides (inches)	Terrain	Remarks
46	Level	Started in potato patch	79	Uphill	Start on old mossy wood road
—	Level	Scuff mark	42	Uphill	
—	Level	Scuff mark	72	Uphill	
—	Level	Scuff mark	50	Uphill	
43	Level		50	Uphill	
46	Level		56	Uphill	
54	Level		52	Uphill	
45	Level		54	Uphill	
50	Level		50	Level	
45	Level		62	Level	
48	Level		85	Level	
69	Level		62	Level	
105	Downhill	Extended over debris	85	Level	
36	Downhill		52	Level	
53	Downhill		56	Level	
—	Downhill		50	Easy downhill	
—	Downhill		48	Easy downhill	
—	Downhill		42	Easy downhill	
—	Downhill		40	Easy downhill	
43	Downhill		43	Easy downhill	
36	Level		45	Easy downhill	
40	Level		52	Easy downhill	
28	Level	Stopped beside tree	55	Easy downhill	
44	Level		80	Easy downhill	Stepped over down-fall
46	Level		30	Steeper	
42	Level		—	Steeper	
45	Level		45	Steeper	
			45	Steeper	
			—	Steeper	Scuff
			—	Steeper	Scuff
			57	Steeper	
Average stride is 48 inches.			Average stride is 55 inches.		

very disturbed, and they barked furiously for long periods. One evening, the father fired a shot in the air with a high-powered rifle to quiet the dogs and to frighten off anyone who might be trying to pilfer equipment. The tracks were encountered by the oldest boy, who was checking the property on the morning of November 7, 1986, following considerable disturbance by the barking dogs during the night.

Our investigation of the tracks indicated the presumed animal had skidded in some of its strides, which tended to exaggerate the true size of the foot-

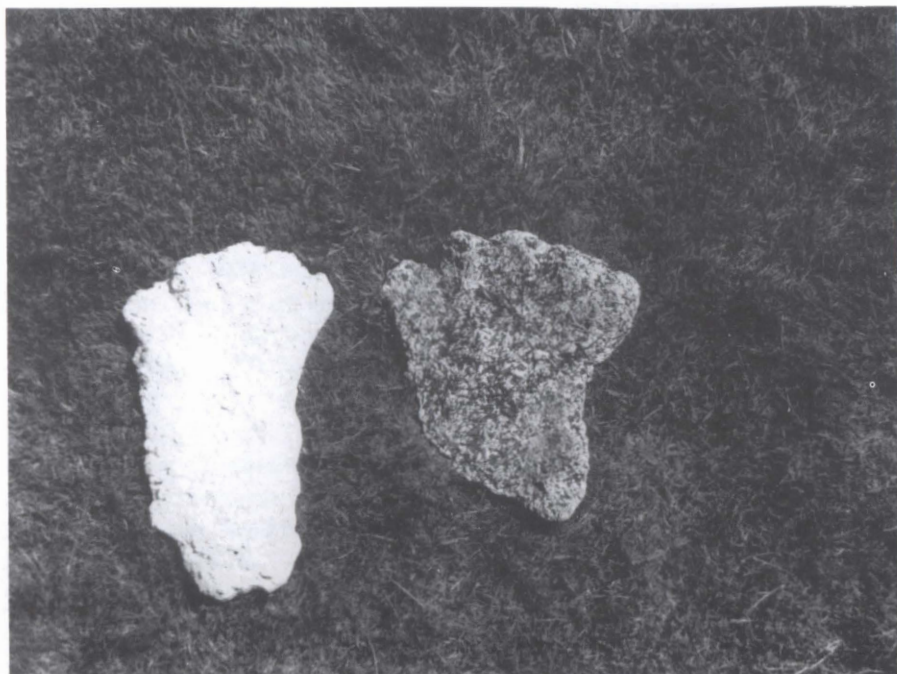


FIG. 1.—Two casts of right foot tracks. Note the similar size of 1st and 5th digits. The cast on right has a 12-inch toe spread, and the heel did not mark. The cast on left has a 9-inch toe spread, and has weight distribution on the entire foot—it is a copy of original cast Grover Krantz used for dermatoglyphic study.

prints. The skid marks created footprints up to 24 inches in length, but the true size of the prints was determined to be about 16 inches. There were two lines of tracks, traveling away from the direction of the cabin, approximately 50 yards apart.

The upper line of tracks started from a potato patch, with no discernible tracks (just scuff marks in the potato patch, in which two small diggings were evident), and traveled about 50 yards, turning to the right, moving uphill, and vanishing at the edge of heavy brush. The lower line of tracks started from a moss-covered wood road, and traveled downhill, making nearly a U-turn back to the wood road, then vanishing.

All measurable prints were examined in both lines of tracks (Table 1). The average stride of twenty prints measured in the upper line was 48 inches. The average stride of twenty-eight prints measured in the lower line was 55 inches. Both lines of tracks exhibited flexible toe spread, and had similar heel and width-of-foot characteristics, leading me to propose that both lines of tracks were made by the same individual animal.

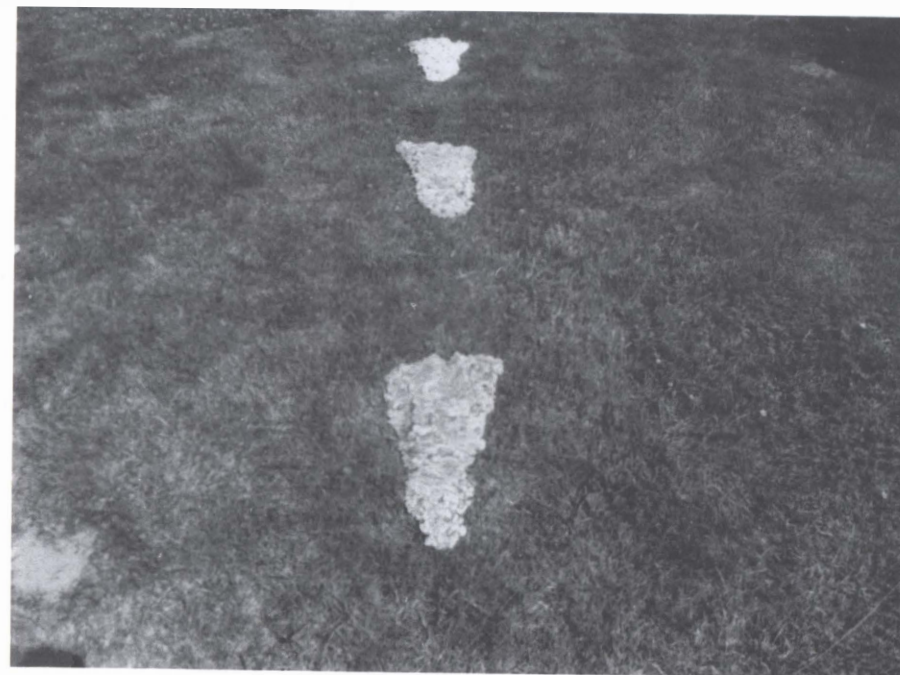


FIG. 2.—Three tracks cast in sequence, showing right, left, and right footprints from bottom to top. Note the lateral extension of 1st digit in the middle and upper print.

The strides ranged from 28 inches to 105 inches in the upper line of tracks. The lower line strides ranged from 30 inches to 85 inches. The skid tracks, which were noted in both lines of tracks, were attributed to saturated ground conditions resulting from heavy rains which had drenched the area during that period. It was noted that the first digit of the foot was sometimes extended laterally to a considerable degree.

Considerable variation in toe spread and toe positioning indicated the animal had a very flexible foot. The widest toe spread (12 inches) was exhibited in a right footprint in which the heel did not mark, although there was a detectable smoothness of the ground litter where the heel had lightly touched down (Fig. 1). This was the only print that displayed all the weight distribution on the forepart of the foot. Toe spread width ranged from 9 inches to 12 inches. Heel width was 4 inches to 5 inches.

In general, the prints revealed widely spread toes, with an acute lateral extension of the first digit, and heavy distribution of weight in the fore part (Figs. 2, 3, and 4). However, a few showed a human-like toe position.

In places where ground litter was deeper, the toes pushed up—or lifted out—considerable material ahead of the print as the foot was raised for the



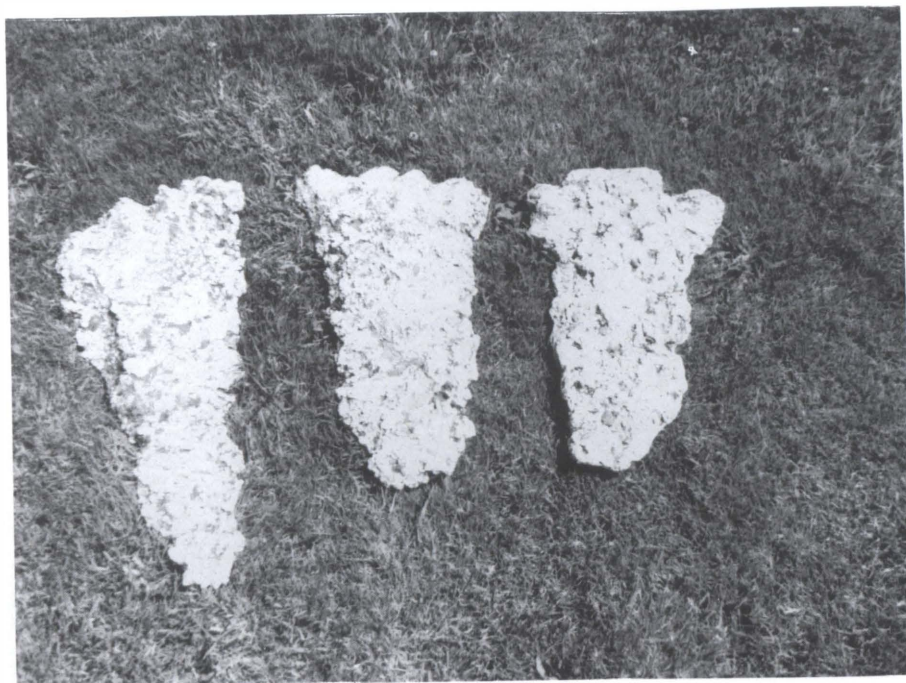


FIG. 3.—The same casts as shown in Fig. 2, with a better view of the variation in movement of the 1st digit.

next step. Around the edge of the entire print, litter and soil were squeezed up one or two inches. The shortest measured pace was 28 inches, where the animal apparently stopped beside a tree. One could speculate that, under the circumstances, this was logical behavior, since a tree trunk would tend to conceal its presence.

#### RESULTS

Several casts of footprints were subsequently examined by Grover Krantz, at Washington State University, who selected one for dermatoglyphic study (Grover S. Krantz, 1983, *Anatomy and Dermatoglyphics of Three Sasquatch Footprints*, *Cryptozoology*, Vol. 2: 53–81). Some dermatoglyphs were present, and cast copies were made (left cast in Fig. 1).

I am not an expert on the anatomy of the foot, so will not elaborate on the subject. However, there are variations in the positioning of digits one and five to a degree that, to my knowledge, has not been recorded in previous casts of presumed Sasquatch tracks.

Susan Cachel has questioned Krantz's identification of the Elk Wallow (Walla Walla) prints as hominid by virtue of the marked adduction of digit



FIG. 4.—Another view of the right foot, indicating variable toe positioning. The heel is not shown on the print on the right due to a shortage of plaster when casting.

one (Susan Cachel, 1985, *Sole Pads and Dermatoglyphics of the Elk Wallow Footprints*, *Cryptozoology*, Vol. 5: 45–54). These prints suggest they were made by a bipedal animal with a very flexible foot, presumably adapted to cope with the rugged terrain of the Pacific Northwest. Possibly some of the digits of the foot could be used for grasping, and the foot would have a thick sole pad for protection. Such flexibility would certainly enable such an animal to more easily scale perpendicular cliffs, as is reported occasionally.

My impression of the animal's behavior at the track site was as follows (which, I confess, is pure speculation): It did not *have* to leave tracks, but was irritated by the noise and confusion of the farm animals, so it plunked its feet around in a "fit of temper." It returned the same night, or the next, and repeated the display. The recent logging of several acres of timber may have had an impact on its foraging activity, and added more aggravation. This might explain why the tracks abruptly started and abruptly ended. I feel confident that Sasquatch seldom leave tracks to be looked at.

It seems to me, the tracks involved in this case study clearly represent an animal that is capable of abundant physical diversity. It can walk lightly on its toes when the occasion arises, or it can take long easy strides heel to toe.

It can slow down when in skid action by spreading the toes, and, likewise, it can use individual toe flexibility for dexterity in climbing steep terrain and scaling mountainous areas.

The animal evidently has a super muscular arrangement to enable its gigantic body to perform with speed and strength. This can only be accomplished by means of enormous muscles attached to stout ankle and leg bones, robust thighs to further aid in stride agility and maneuverability, and a very massive pelvic-lumbar area for muscle attachment and lower back strength.

I think it would be precarious to make comparisons with known primates in an attempt to classify this unknown animal. It appears to have physical properties and capabilities unlike any known animal.

#### FUTURE PLANS

The author plans to continue searching in the State of Oregon, and possibly elsewhere, for quantitative data that will shed more light on Sasquatch behavior. Needless to say, absence of good behavioral data is not an easy start.

## INVESTIGATIONS AND SONAR TESTING AT LAKE CHAMPLAIN, 1987

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#### INTRODUCTION

In July, 1987, the authors collaborated on fieldwork at Lake Champlain. This work grew out of previous seasons' activities coordinated by Smith regarding the supposed large, unknown "Loch Ness-like animals" reported at the lake and popularly referred to as "Champ" (Richard D. Smith, 1986, *Investigations and Systems Tests in the Lake Champlain Basin*, 1986, *Cryptozoology*, Vol. 5: 85-88).

The intent of the 1987 work was: 1) To test a shore-based chart recording sonar system capable of extending a sonar beam horizontally across the lake's central basin; 2) to make additional vertical sonar chart recordings of the lake basin from a boat; 3) to gather any information on previously unrecorded Champ sightings, and to relay such information to the Lake Champlain Phenomena Investigation (LCPI); and 4) to gather any information of interest to professional scientists and/or local historians.

Our 1987 fieldwork consisted of 14 days on site. Six days were spent in the execution of the primary sonar activities, which were directed by Konrad. The remaining days were devoted to the additional activities, coordinated by Smith. Assisting in operations were Ike and Lois Blonder, Phil Keller, Werner Larsen, Wendy Lathrop, Margaret Light, and Gary Mangiacopra. Katherine Teetor was most helpful during inquiries on the Vermont side of the lake. Morris and Ellin Glenn kindly allowed us to deploy the land-based sonar from their property near Essex, New York.

Arthur Cohn, director of the Lake Champlain Maritime Museum, Basin Harbor, Vermont, and Joseph Zarzynski, director of the LCPI, provided valuable suggestions during our selection of sonar work sites. We also acknowledge the encouragement and support shown by Mr. Blonder, Ms. Light and Robert Durant.

#### NARRATIVE DESCRIPTION

The principal sonar was a 112 kHz unit designed by Konrad, used in conjunction with a line scan OSR-219 recorder. The sonar produced a source level of 235 dB with respect to one micro Pascal at one meter. The transducer, which served both for transmitting and receiving, was 12 inches square and weighed 60 pounds. Approximately 5 kilowatts of pulse power was fed to



the transducer during the 5 millisecond transmit period. Five hundred feet of coaxial cable connected the transducer to the sonar unit ashore.

A dive mechanism to scan the transducer vertically through 15 degrees was designed by David Olsen. We had intended to mount the transducer on a steep underwater drop-off immediately adjacent to the deepest central channel of the lake. However, logistical considerations led us to opt for a more easily worked site near our base camp during this season's testing of the system. The transducer was suspended 15 feet beneath a canoe moored in 30 feet of water, and 350 feet off a shore that sloped gently into the central channel. Depth contours taken of this location using the secondary sonar unit (an Apelco/Raytheon 201-C chart recorder) revealed that the transducer's narrow 4.5 degree beam would most likely expand above the bottom topography, and not cause early intersection of the signal with the bottom before reaching into the central channel (assuming an isothermal water temperature profile). The scanning motor was therefore not attached to the transducer. A more fixed mounting for the transducer would have been desirable. However, the moored canoe's position shifted only gradually hour-to-hour with prevailing winds, so this deployment was quite adequate for our tests.

As in previous seasons, shore watching was not emphasized, but expedition members carried a variety of still, motion picture, and video cameras in the event of a chance Champ sighting.

Scans with the Apelco sonar unit sweeping vertically from a moving boat were carried out near Essex, New York, Thompson's Point, Vermont, and Basin Harbor, Vermont.

#### RESULTS

The primary sonar system was operated for several periods over four days. The results were not satisfactory due to two factors. We had anticipated that water temperature differences between the epilimnion and hypolimnion strata might act to refract the horizontally aimed sonar signal downward. This proved to be the case. We were able to record only at a distance of some 1,250 feet, not at the equipment's three-mile potential. Ray trace calculations based on in-situ temperature versus depth measurements—together with the historical data of Henson and Potash (personal communication)—generally supported the observed sonar range limit. An unanticipated problem was that high humidity caused the chart paper to bend and soften, causing the recording styli to tear into the paper on several occasions. Although we appear to have recorded some aquatic life, we were unable to satisfactorily calibrate the system on site. Any interpretation of results from the primary sonar would thus be speculative. The secondary unit recorded fish and what appear to be two known boat wrecks.

Two other results are presented here for the interest they may have for cryptozoological fieldworkers in general.

As in previous seasons, information on unrecorded Champ sightings was gathered. Information on three sightings (one from this season, two from years past) was relayed to the LCPI, and an interview concerning a fourth was conducted at Zarzynski's request. It should be stressed that the LCPI is vigorous in its efforts to obtain and record eyewitness testimony. Even in the presence of thorough, ongoing efforts, it is still possible to uncover hidden accounts of "hidden" animals because: 1) researchers and media representatives do not automatically hear of all sightings, and 2) eyewitnesses are often too reticent or too busy to answer inquiries immediately. Thus, to the oft-stated skeptical inquiry "If they exist, then why aren't these animals seen more often?," one may answer: "Apparently they *are*."

Although no sightings were made of Champ animals, Smith photographed a beaver that had entered the Charlotte, Vermont, anchorage area from an adjacent estuary, and had momentarily appeared at the surface. This incident demonstrates that, if one wishes to photograph elusive animals, it is not sufficient to simply "have a camera along." The camera must be on one's person when near the animals' supposed habitat.

#### FUTURE PLANS

The authors believe there will be great value in establishing a sonar "curtain" across the lake's central basin, and recording for periods of twenty-four hours or longer. Large, free-swimming animals, if they exist in Lake Champlain, could be detected by this method. Clues to their behavioral patterns could thus be gleaned. Optimal deployment will be necessary, and the problems encountered this season will need to be addressed.

A small research vessel was acquired this year. The strategy of underwater video camera deployment tested last season (see Smith, above) will be resumed in 1988. Konrad is also designing a system of imaging sonar which, if successfully developed, will greatly increase our ability to "see" in the lake's dark, deep waters.

Liaison will be maintained with the LCPI, professional scientists, and local researchers.

## Book Reviews

*Cryptozoology*, 6, 1987, 88–107  
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*Les Survivants de l'Ombre* [Survivors of the Shadows]. By Jean-Jacques Barloy. Arthaud, Paris, 1985. 267 pp. 92 FF. (p.).

These “survivors of the shadows” are, of course, the unidentified beings which cryptozoologists hope, someday, to identify. In this book aimed at the general public, Jean-Jacques Barloy presents all of the most famous cryptozoological cases, from the Great Sea Serpent to the Sasquatch and the Yeti, as well as samples of what could be called “historical cryptozoology,” with several chapters devoted to more or less well-known enigmas of the past.

One of the most famous of these enigmas is that of the “Beast of Gévaudan,” one of several “beasts” which terrorized whole regions of France from the seventeenth to the eighteenth century, and have even found echoes in recent newspaper reports. The Beast of Gévaudan killed about one hundred people between 1764 and 1767, before it was itself killed—but not identified. Barloy goes to great lengths to exonerate wolves (often considered as the culprits), and favors the frequently repeated idea of human intervention (from sadists to persons with obscure political motives). However, his statistical research on the subject is, to say the least, inconclusive (and it seems unlikely that witnesses of attacks by the Beast could really have mistaken a man in an animal skin for a true animal, whether a wolf or something more unusual—the only remaining possibility being that of trained dogs or wolves).

Barloy also devotes a whole chapter to “wild children” and “wolf children” (and similar cases of human children supposedly raised by wild animals). This is not, properly speaking, a cryptozoological problem, but it may have a bearing on some reports of “wild men.”

The rest of the book is devoted to more usual cryptozoological problems—if cryptozoological questions can be said to be usual. Barloy’s book obviously owes much to the works of Bernard Heuvelmans, although this debt is probably not sufficiently acknowledged, and it often reads like a not very successful digest of *Sur la Piste des Bêtes Ignorées* (*On the Track of Unknown Animals*), one of the main differences being that Barloy is not as reliable in the choice of his sources as is Heuvelmans. This kind of cryptozoology,

essentially based on reports from a variety of sources, is largely a matter of sound bibliography and sensible critical appraisal of the sources. Unfortunately, it seems that Barloy has mainly sought to cram as many mysterious episodes as possible into the pages of his book, without much regard for the credibility of some of them. The result is that the book is, in some ways, too much like its rather ludicrous cover, which shows three enormous and fanciful monsters threatening a helicopter.

Too many strange stories are recounted without any mention of a bibliographical reference. There is, for instance, an especially silly tale of the shooting in the Congo, in 1929, of a blond ape-woman—who turned out to be the daughter of a Mrs. Bertelli, the Swedish wife of a Danish explorer—and an ape-man. No indication whatsoever is given as to the origin of this story, which reads like the worst kind of adventure fiction, and one may wonder why the author has seen fit to include it among more convincing reports on mysterious African primates. Moreover, when sources are mentioned, they are sometimes rather suspect (a hardly believable story of a giant jellyfish is taken from *Psychic Australian*, probably not the most reliable source of sound zoological information).

Many parts of the book do make much better reading, but this frequent mixture of “serious” cryptozoological research and what often amounts to sensational nonsense is irritating, and not likely to improve the image of cryptozoology on the part of the scientific community. The book is easy to read and likely to appeal to a fairly large public, but it can hardly be called a successful experiment in popular cryptozoology. It is, of course, not the only book in which a discussion of cryptozoological problems is marred by the inclusion of unbelievable tales and implausible hypotheses for the sake of more exciting reading. But authors of popular cryptozoology books should understand that one of the things cryptozoology needs is scientific respectability, and that their too frequent unbridled quest for the sensational seriously damages the impression that practitioners of the more established scientific disciplines get of cryptozoological research.

The illustration of Barloy’s book consists of rather simple hypothetical depictions of cryptozoological creatures, and reconstructions of fossil animals, some survivors of which may be the basis for some cryptozoological reports. A number of them, frankly, are ridiculous, one of the worst being the “giant sloth which lived in Argentina until a recent epoch,” depicted on p. 143 as an enlarged version of a modern three-toed (tree) sloth, with its short face and reduced tail. A good skeleton of a giant ground sloth (*Megatherium*) resulted in one of the first fossil vertebrates to be scientifically described, at the end of the eighteenth century, with the result that reliable reconstructions of ground sloths have been published in dozens of paleontology books during the last two centuries. The perusal of any good textbook on vertebrate paleontology would have shown the author and the artist that



extinct ground sloths, with their strong tails and massive heads, were quite different in appearance from their small, arboreal, extant cousins.

Had he been more strict and discriminating, Barloy could have provided the French-speaking public with a fairly good review of cryptozoological problems. As it is, his book is bound to give an inaccurate and unfavorable impression of what serious cryptozoology is.

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*Quaternary Extinctions: A Prehistoric Revolution.* By Paul S. Martin and Richard G. Klein (eds.). The University of Arizona Press, Tucson, 1984. 892 pp. \$65 (c.).

In the late 1970's, when Paul Martin first told me about plans for this new, multi-author volume, nobody envisioned that the final product would turn out to be what it is—least of all Martin; but the years went by, more authors joined the project, and they all kept adding to their contributions.

It can now be stated with confidence that *Quaternary Extinctions*, with contributions from specialists world-wide, is the most comprehensive work on the topic to date—and will probably remain so for a decade or two. (The Quaternary encompasses the Pleistocene—the Ice Ages—from about 1.7 million years B.P. to about 10,000 years B.P., and the Holocene, from about 10,000 years B.P. to the present.) The book is handsomely produced, has 38 chapters written by 47 authors, and is divided into seven major sections, each with its own introduction. Illustrations and tables are profuse. Every chapter has its own references, and there is a good general index at book's end.

This is Martin's second major foray of this kind. In 1967, he and H. E. Wright, Jr., edited *Pleistocene Extinctions: The Search for a Cause*, published by Yale University Press, which was based on conference papers. That book had 453 pages and 17 chapters, as well as a very useful 62-page bestiary of Pleistocene vertebrates, with taxonomic descriptions and illustrations. The new book could be called a sort of sequel to the 1967 volume, but with extensive additional information, as much more fieldwork—and theoretical work—has been conducted in the intervening years.

Before reviewing the new volume in more detail, perhaps we should examine some of the theoretical background of Pleistocene extinctions. Martin is best known for his "overkill" hypothesis, which he presented in the above 1967 Yale volume. However, it was a 1973 paper in *Science* (Vol. 179: 969–

74) that exposed his thinking to a much wider scientific audience. The paper was entitled "The Discovery of America," and, in a sense, nothing has been the same since.

Simply put, the basis of Martin's overkill hypothesis is as follows:

1. Archaeological evidence indicates that a wave of human migration from Asia to the Americas—across the then Bering land bridge—occurred about 11,000 years B.P. (There is increasing evidence that other migrations occurred prior to that, perhaps as early as 20,000 or 30,000 years B.P., or even earlier, but that controversy does not directly concern us here.)
2. Paleontological evidence indicates that most of the Pleistocene megafauna (large animals, over 100 lb. in weight) in the Americas became extinct around 11,000 years B.P. Therefore, the megafauna were a) killed off abruptly by proficient Asian (Clovis) hunters who found an American paradise previously untapped by humans, or b) the timing of the two events was essentially coincidental, and the megafauna became extinct primarily due to other causes, such as climatic/environmental changes.

Martin's overkill hypothesis predicts the former, and envisions human groups continually migrating south, from what is now Edmonton, Canada, to what is now southern Argentina, wiping out all the large animals they encountered during a fast-moving, 1,000-year sweep to Patagonia. The correlation of the appearance of what he thinks were the first humans in the Americas and the disappearance of most of the megafauna, Martin thinks, is simply too much to be a coincidence.

Martin's hypothesis has been criticized on various fronts, one from archaeologists: there is remarkably *little* evidence of Clovis hunting and butchering of megafauna in the archaeological record. Martin's response is that the overkill was rapid—an ecological "blitzkrieg"—and its relatively short duration of about 1,000 years (and probably only decades in specific geographic areas) simply did not permit enough time for the event to be substantially visible today in the archaeological record. The reason the blitzkrieg was made possible was probably the megafauna's lack of fear of the relatively small and harmless-looking bipeds—humans—they had never encountered before. The extinct megafauna in question comprises such animals as mammoths, mastodons, giant ground sloths, glyptodonts, taxodonts, camels, horses, and a host of other mammals and birds with which they were ecologically interdependent.

Another problem with the overkill hypothesis is the lack of evidence for megafaunal overkill in Australia—a continent to which Martin later applied his model. The first humans arrived in Australia between 30,000 and 40,000 years B.P., and the extinctions, according to some authorities, occurred

between 26,000 and 15,000 years B.P. The question—if Martin is correct—is why didn't the early Australians also rapidly hunt their large animals to extinction? Martin proposes that they did, and that the extinctions actually occurred prior to 30,000 years B.P. Australia, however, remains one of the main nagging problems with overkill.

Also, as is usual with such grand, sweeping schemes, there is probably more to the whole problem than meets the eye. Perhaps Martin is right, but only in the Americas; perhaps the early Australians were not much of a hunting society; perhaps late Pleistocene climate changes were already affecting megafaunal communities detrimentally in some parts of the world, and the arrival of humans was an added factor which produced different results in different world regions.

Whatever the case may be, and regardless of one's position on the subject, *Quaternary Extinctions* is the book to turn to for information. There is much here also for the cryptozoologist, although Martin himself—as I know from personal experience—is no supporter of cryptozoology. This may be because Martin's hypothesis predicts the *complete* extinction of the American megafauna—within a thousand years—*everywhere* in the Americas. This includes not only the herbivores such as the elephantids and ground sloths, but also all the other major players in the game. Like falling dominoes, a cascading collapse of trophic levels would have destroyed a sensitive ecological network. In short, a total mess. Even mighty *Smilodon*, the large saber-toothed cat, would have perished, as the predatory/prey game in which he was a participant would have ended.

Evidence for ground sloths (or other megafauna) surviving well into the Holocene, perhaps even to the recent past—or even to the present—certainly does not support Martin's overkill hypothesis. (Neither does growing archaeological evidence of pre-Clovis arrivals in the Americas—prior to 11,000 years B.P.—and Martin is also very dubious of that evidence.) On the other hand, I don't think that the discovery of a surviving species of Pleistocene megafauna (or the discovery that a species—such as the mammoth—lived far beyond its presumed extinction, perhaps until the past millennia or two) would be a real blow to Martin's hypothesis; perhaps some species inhabited—or moved to—more inaccessible areas, out of the reach of most hunters. The overkill/blitzkrieg hypothesis and the concept of cryptozoology are certainly not mutually exclusive. Martin, however, insists that, for his model to apply, *no* hunting humans could have arrived much before 11,500 years B.P., and *no* megafauna could have survived much beyond 10,500 years B.P.

Following a Note from the Editors, Donald Grayson leads off *Quaternary Extinctions* with a 35-page review of nineteenth century thinking on the subject, which is extremely useful in putting today's debate into a historical perspective. Such names as Cuvier, Lyell, Fleming, and Wallace form part

of nineteenth-century thought. This is followed by Elaine Anderson's magnificent 50-page mammalian bestiary, entitled "Who's Who in the Pleistocene," indispensable for anybody who wants a handy reference on the subject.

Chapter 3, by Larry Agenbroad, reviews the distribution of mammoth in the New World. Most remains come from Alaska, the Canadian prairie provinces, and the central and southwestern U.S.A.—and Florida. The evidence dwindles in Latin America, either because mammoths were few or nonexistent, or because sites have not yet been found. Beyond Mexico, the southernmost accepted site is in El Salvador. Timewise, a total of 78 datable sites gives the most recent dates as between 10,000 and 11,000 years B.P., in support of Martin. Some dates give less, the most recent being 7,670  $\pm$  150 years B.P., but dating problems could be involved.

Chapters 4 through 8 constitute "a close look at significant sites." Agenbroad reviews the Hot Springs, South Dakota, mammoth site. Ruth Gruhn and Alan L. Bryan review the Taima-Taima site in Venezuela, which has provided remains of horse, bear, glyptodont, mastodont, and others. Miles Gilbert and Larry Martin discuss Natural Trap Cave, in northern Wyoming. The cave is actually an 85-foot-deep sinkhole into which animals have fallen over an extended period of time, and are now available for study in a clearly stratified deposit. The site is "the longest and most extensive continuous record of Late Pleistocene biota in the northern Rockies," and includes many mammals, from grazers such as camels and horses, to carnivores such as the North American lion and cheetah. The most recent date, again, is 11,000—12,000 years B.P.

Chapter 7, by Arthur Phillips III, discusses the extinction of the Shasta ground sloth, based on pack rat midden studies in Rampart Cave, in the Western Grand Canyon. The latest radiocarbon date for a sloth dung ball is 11,140  $\pm$  250 years B.P. In Chapter 8, Leslie Marcus and Rainer Berger discuss the Los Angeles site of Rancho La Brea—the famous tar pits. An estimated 3,400 individual larger mammals, and 5,845 individual birds are now preserved. The dire wolf and *Smilodon* make up 48 and 30 percent respectively of all large mammals. The latest date for *Smilodon* is 11,130  $\pm$  275 years B.P. The latest date for a large mammal is 5,270  $\pm$  155 years B.P.—but it is for a grizzly bear, which is still with us today.

The next section of the book, Chapters 9 through 15, addresses geophysical-climatic hypotheses for late Pleistocene extinctions. David Webb gives the "big picture" of North American extinctions during the past 10 million years. Philip Gingerich also looks at the bigger picture, showing how the early Pleistocene saw an unusually high rate of genera originations, making the subsequent extinctions a "natural sequel." Russell Graham and Ernest Lundelius, Jr., emphasize the "disruption of evolutionary interactions" between the megafauna and plant communities due to climatic changes, and the resulting ecological "disequilibrium" which could lead to mass extinc-



tions. The late John Guilday discusses the response of species to environmental change (they must move, adapt, or perish), and acknowledges that both man and climatic change affected ecosystems, "but what effect one would have had in the absence of the other is untestable."

Dale Guthrie presents a 30-page chapter on biotic changes which reduced faunal diversity, body size, and range, leading to extinctions, and Richard Kiltie has an equally intriguing chapter on the impact of climate change on seasonality and reproductive biology. He points out that many of the large mammals which became extinct had long gestation periods, as do many modern mammals in tropical zones, making the species more vulnerable during climatic change. James King and Jeffrey Saunders round this section up with a chapter in which the mastodont is used as a case study in extinction through "environmental insularity." The coniferous habitat of mastodonts in western Missouri was disappearing due to climatic change, and although the animals tried to adapt through "self-regulation," they eventually became locally extinct. The authors envision coniferous refugia for mastodonts in ever-expanding deciduous forests, both the coniferous "islands" and the mastodonts dying together no later than 13,000 years B.P.—and with presumably no help from human predators.

The next major section presents six chapters relating the extinctions to a human agency. Vance Haynes discusses specific sites, their datings, and the implications. Besides discussing the stratigraphic evidence, he cites paleohydrological data from the late Pleistocene indicating that, at least in the U.S. Southwest, aridity may have contributed to megafaunal extinctions—directly and indirectly: rare watering holes sought by large mammals may have become deathtraps, with Clovis hunters awaiting their inevitable arrival.

Chapter 17 is Paul Martin's own opus, and, in a sense, it is the heart and soul of the book. Entitled "Prehistoric Overkill: The Global Model," the 50-page chapter attempts to consolidate Martin's current thinking and position. He lists eight attributes of late Pleistocene extinctions, and goes on to review the extinction record in North America, South America (one shortcoming of the book is its scant coverage of South America, but this is acknowledged), Australia, Africa, Europe, and oceanic islands.

Martin states: "... the youngest remains of the extinct [megafaunal] animal approximate 11,000 radiocarbon years ... the time of unmistakable appearance of Paleo-Indian hunters. ..." He points out how, while small mammal extinctions decreased through the Plio-Pleistocene, large mammal extinctions increased, a difference which is "in accord with the model of human impact as a size-selective force, operating effectively on the larger (and more slowly reproducing) fauna." In other words, if climatic change was responsible, one would expect the extinctions to have been less selective, and the record would show both small and large mammals to have perished

in about equal proportions. Martin then asks, if blitzkrieg is correct, how did some large mammals—the ones with us today—survive? He points out that almost all the surviving genera had an Asian origin, and thus were probably already more adapted, through co-evolution, to survive man's predation.

No Neotropical genera are now found in North America, but some North American genera survive today in Central and South America, and Martin sees this as further evidence of blitzkrieg. Early American hunters could have easily killed off these animals in the North American plains and woodland, but such animals could have remained better hidden in the southern tropical forests. By the time the hunters' descendants reached the open biomes of northern Argentina—several hundred years later—their cold-climate hunting capability would have been lost, and would have to have been re-learned, giving the North American fauna in the southern hemisphere more time to adapt to human presence. All these arguments are quite fascinating and compelling. As for the *native* southern fauna, South America, he says, became their "graveyard . . . no other continent lost as many animals in the late Pleistocene . . . it is difficult to imagine an assemblage of large mammals that would have been easier to track, hunt and exterminate than the numerous kinds of ground sloths and glyptodonts that disappeared down to the last species. . . ."

(In a curious comment on the Chacoan peccary, *tagua*, discovered in Paraguay in the 1970's, Martin states that "it is a 'living fossil' of the Holocene, not of the Pleistocene." Perhaps his concern is that this pesky peccary's survival could undermine overkill, despite the fact that it is [just] under the minimum 100 lb. in weight to qualify as a megafaunal animal. Its description by Ralph Wetzel [*Science*, Vol. 189: 379–81], and his subsequent publications [*Annals of the New York Academy of Sciences*, Vol. 288: 538–54; *Bulletin of the Carnegie Museum of Natural History*, No. 3: 1–36; *Carnegie Magazine*, Vol. 55: 24–32] clearly define it as a Pleistocene genus, *Catagonus*. The animal is even included in Anderson's Pleistocene bestiary in Martin's own book. Martin's point seems to be that osteology of a related species was once found in a pre-Spanish archaeological site. Obviously, any Pleistocene taxon alive today would necessarily have had to have lived *through* the Holocene—but does that make it a Holocene animal?)

Moving to Australia, Martin acknowledges the problem for blitzkrieg, but calls the evidence for megafaunal survival beyond human arrival (at least 30,000 years B.P.) "sparse and scrappy." He admits that his hypothesis "can be rejected . . . if a 'late' extinction chronology proves valid, that is, if the large extinct mammals are found to have maintained sizeable populations long after initial human invasion. . . ." Only future radiocarbon dating can resolve this problem.

Martin also discusses extinctions in Africa, Europe (including Mediterranean islands), and oceanic islands, and their implications, but this review

must move on. In his conclusions, Martin vigorously defends blitzkrieg overkill—which is criticized in other chapters in this, his own book. He emphasizes how, “on a global scale, the late Pleistocene extinction patterns appear to track the prehistoric movements and activities of *Homo sapiens*. . . . The moderate loss of animals in Afro-Asia can be related to the gradual history of human spread which allowed an ecological equilibrium to develop. The loss of the great majority of large animals from North America, South America, and Australia can be related to sudden and severe human impact on these continents, which were previously unexposed to evolving hominids.”

Following Martin, Jerry McDonald discusses biome changes in North America following the Pleistocene-Holocene transition, showing that biological productivity actually increased significantly. Applying r- and K-selection theory to the problem, McDonald proposes that the K-strategist large mammals would have endured “had not a destabilizing process [Clovis man] appeared in the selection regime.” “The large mammals,” he states, “were more sedentary, territorial, dispersed, and individualistic, qualities rendering them vulnerable to destructive hunting pressures. . . .” Thus, he states, 67 percent of North America’s mammal genera died.

Jim Mead and David Meltzer then tackle the critical question of radiocarbon dating, upon which *all* theoretical work is based (if datings are wrong, all theories based on them are also wrong). They analyze 233 C<sup>14</sup> datings from 150 sites, which represent nearly all the published data (thousands of additional excavated late Pleistocene vertebrates have simply not been dated). Seventy percent of the datings fall in the 11,000–14,000-year B.P. range, but this simply reflects the tendency by many to analyze younger material in attempts to determine the latest occurrence of a taxon. Animals dated at significantly beyond Martin’s proposed extirpation by man include horse and camel, at  $8,240 \pm 960$  years B.P.; mastodont, at  $5,950 \pm 300$  years B.P. and  $8,910 \pm 150$  years B.P.; mammoth, at  $4,885 \pm 160$  years and  $5,985 \pm 210$  years B.P.; and peccary (*Playgonus*), at  $4,290 \pm 150$  years B.P. However, quality ranking and statistical analyses by the authors demonstrate that, invariably, the later dates are, for various reasons, unreliable. In short, despite what one may read in both scientific and popular publications, there is not a single dating of a major megafaunal taxon beyond about 10,000 years B.P. that is considered truly reliable. This will be sad news for cryptozoologists—but cryptozoology, of course, is based on other kinds of evidence than radiocarbon dating.

“Simulating Overkill” is a paper by Stephen Whettington and Bennett Dyke, expanding on a 1975 paper by James Mosimann and Paul Martin in *American Scientist*. Their computer modeling apparently results in a lesser predation rate by man than predicted by Mosimann and Martin. My own view is that the less said about computer modeling the better.

An interesting switch in topic occurs in Chapter 21, when David Steadman and Paul Martin review late Pleistocene bird extinctions in North America. These include teratorns with 16-foot wingspans (and which some cryptozoologists have proposed survive today as “thunderbirds”), condors, vultures, eagles, and hawks. The authors conclude that human agency had a negligible direct impact on bird extinctions. However, they attribute a disproportionate number of scavenging bird extinctions to *indirect* human impact—there were eventually no megafauna for them to scavenge on. Also, the lack of massive bird extinctions indicates, they think, that the large mammal extinctions were caused by man and not by climate change.

The fifth major section of the book discusses extinctions in Asia and Africa. Such case studies can shed light on the American extinctions. A 34-page chapter by Soviets Nikolaj Vereshchagin and Gennadij Baryshnikov review Pleistocene extinctions in their country, particularly the mammoth, the cave bear, the cave hyena, the cave lion, the woolly rhino, and giant deer. A questionable C<sup>14</sup> date on woolly rhino collagen once gave 800–1,000 years B.P., and “fresh” rhino bones “with the distinct odor of collagen” are said to repose at Kazan University. An interesting history is given of human hunting in the U.S.S.R. from the Paleolithic to this century, but the authors subscribe to biome transformations as the principal cause of Eurasian extinctions, with direct human impact occurring only in recent millennia.

Liu Tung-Sheng and Li Xing-Guo discuss mammoths in China, where there are no less than 157 fossil sites. No fossils more recent than 20,000 years B.P. have yet been uncovered, but there is much evidence of human predation. Israel’s relatively few extinctions are reviewed by Eitan Tehernov, who presents many tables and graph figures on Pleistocene mammals in the Levant. This is followed by an examination of African extinction by Richard Klein, the book’s co-editor. Africa has a much different history from that of the Americas, and had far fewer large mammal extinctions—perhaps because the megafauna gradually developed escape strategies *during* human evolution, rather than suddenly being exposed to human intelligence and cunning, as happened in the Americas. This chapter, nevertheless, will be valuable for those interested in African prehistory.

Then comes Madagascar. One of the most interesting places on Earth biogeographically, Madagascar is discussed by Robert Dewar, who reviews Holocene extinctions. It will be a surprise to many to learn that no Pleistocene—or even Cenozoic—fossil sites exist at all in Madagascar. Everything is extrapolated from subfossil deposits, which are poorly described, and extant species. The principal mammal fauna are the prosimian lemurs. Seven of the 17 Holocene genera are now extinct, and two extant genera have each lost one species. All the extinct lemuroids were larger, probably diurnal, and some were ground-dwelling—making them more susceptible to human predation.



Also of interest are Madagascar's now extinct ratites, the large, flightless "elephant birds." The largest, *Aepyornis maximus*, stood almost 10 feet tall. Dewar comments on the many eggshell pieces still found today on coastal beaches, and he believes these are the remains of human meals, as "these ponderous birds would have been easy prey to arriving humans." It is curious that, although it is the fourth largest island on Earth, humans discovered it only within the past two millennia, probably about 500 A.D., Dewar mentions the reports by Admiral Etienne de Flacourt in 1658 of "an ostrichlike bird," indicating the persistence of *A. maximus* at least into the seventeenth century. However, because of the lack of subfossil material in the known archaeological sites, he considers the possibility of the bird's persistence beyond 900 years B.P. as "unlikely."

What is not mentioned is that other evidence subsequently supported Flacourt's reports—all of which is discussed in Bernard Heuvelman's 1958 classic *On the Track of Unknown Animals*. Also not mentioned is the fact that all of this occurred *before* physical evidence was found and the bird was described in the 1850's and 1860's. The earlier eyewitness reports, therefore, are not likely to have been colorful stories based on an already described but extinct bird. One is left to wonder, as Heuvelmans himself has pointed out, why the naturalists of the day did not investigate the reports, instead of scoffing at them. Had they done their scientific duty, the species, if still alive, might have been saved from extinction, and, if protected, could conceivably have survived until today. And now, sadly, we see the same attitude expressed again—three centuries later—in the latest scientific work.

Nine chapters make up the next section of the book, dedicated to Australasia and the Pacific, and we're not done with bestiaries yet. Peter Murray's 28-page bestiary is an Australian counterpart of Elaine Anderson's, but with more extensive individual discussions and fine drawings. There is a good discussion of *Thylacoleo*, believed extinct since the late Pleistocene, but no mention is made of its possible persistence to the present in the form of the Queensland Tiger—reports of which the author is probably quite unaware of. As for the thylacine, the mainland form of which supposedly became extinct as recently as 3,000–4,000 years B.P., recent mainland eyewitness reports are also not mentioned, and even the Tasmanian form is called already extinct. Duncan Merrilees discusses faunal remains from several sites in extreme southwestern Australia, including those of thylacine, but this author also seems unaware of many recent thylacine eyewitness reports right from the same area.

David Horton presents a 41-page chapter which proposes climatic change as the primary cause of the extinction of at least the megafauna of Australia, the driest continent on Earth. Horton lists and maps all 167 Australian fossil sites, generically and individually, on no less than 30 country maps. He presents several extinction models based on expanding aridity, reviews hunt-

ing strategies in arid zones, and concludes that "no sites have been found that have the characteristics of a mass-kill by humans," and that "the 'mass killing' system was never in effect in Australia." In direct contradiction to Martin's overkill, he believes his climatic models are probably applicable world-wide.

The next chapter, by Geoffrey Hope, discusses climatic and vegetational changes in the Australian Pleistocene. Rainforest reduction began about 35,000 years B.P., at about—or soon after—the arrival of *Homo*, and a cold, arid steppe biome became very extensive. An extremely arid period occurred between 16,000 and 12,000 years B.P. This is followed by Peter Kershaw's chapter on Australian plant extinctions (which introduces the term "megafloora"). Habitat change as contributing to extinctions is proposed.

Chapters 32 through 34 will be of particular interest to cryptozoologists, as they concern those New Zealand ratites, the moa birds. The first chapter is by Michael Trotter and Beverley McCulloch, who provide a concise overview of the moa extermination by the first human settlers, the Polynesians, who arrived about 1,000 years B.P., only about 800 years before European settlement. Evolving as terrestrial browsers for many millions of years, and without outside threat, these giant flightless birds, the largest species of which (*Dinornis giganteus*, formerly *D. maximus*) stood 12 feet tall, suddenly became extremely vulnerable to human predation, and they were thought to have low reproductive rates. What nature created in tens of millions of years, man eradicated in a few hundred years. We also learn that these early human invaders were not the noble savages—interacting in harmony with their new environment—that many writers have portrayed them to be. The first New Zealanders, it turns out, thoughtlessly burnt as much forest and killed as much wildlife as met their immediate needs.

The authors then present the results of 27 radiocarbon dates on moa bone, the oldest being  $1,010 \pm 50$  years B.P., and the most recent being  $421 \pm 55$  years B.P., but the authors seem convinced that the extinctions occurred before that. However, in a true cryptozoological spirit, and unlike Dewar with Madagascar, they acknowledge—when discussing European eyewitness reports—the possibility of the survival of some of the smaller moa species into the nineteenth century. "Possibly some [reports] may have been true," they state; "the odd, small, isolated community of moas could well have survived the main wave of extinction by several centuries, although it is most unlikely that any are now alive."

The same sentiments are expressed by Atholl Anderson in his own chapter on moa extinctions. He does not reject a  $230 \pm 60$ -year B.P. dating, as do Trotter and McCulloch. He also mentions some moa bones and feathers in a "post-European context." Finally, he discusses the many eyewitness reports from the nineteenth century, stating that "many of the accounts are quite fantastic, but some are plausible." He views the moas—which he believes

numbered at least half a million in their heyday—to have been killed off through hunting by 300–200 years B.P., but “some moa could have survived in the most remote western districts until the advent of European settlement.” After a close analysis of all the data from 117 known sites, however, he finally proposes that full extinction occurred “immediately before European settlement,” which essentially refutes all the European eyewitness reports; this is peculiar reasoning, as extending the final extinction of some of the smaller moas by just one more century would nicely accommodate at least the reliable European reports.

Chapter 34, by Richard Cassels, addresses the question of faunal—principally avian—extinctions in New Zealand (“the last large habitable land mass in the world to be discovered and colonized”) and southwest Pacific Islands (east of New Guinea and west of Easter Island). On the moa, Cassels also mentions the nineteenth century eyewitness reports, and admits the possibility “that one species did survive until then” (European settlement). He concludes that the moa’s “absence from late-period archaeological sites cannot prove that the moas were extinct; nevertheless, it is at least strongly suggestive.” Eleven additional pages are dedicated to pre-European extinctions of Pacific birds. Causes of extinction are thought to be hunting, forest burning, dog/rat introductions, and natural changes.

Storrs Olson and Helen James round out avian discussions with a look at Hawaii, where it is now certain that Polynesians also exterminated large numbers of species. As with New Zealand, Hawaii was a perfect ecosystem for the evolution of flightless birds, and they, in turn, became an easy food source—while they lasted—for the invading Polynesians in 500 A.D.

The final section—yes, the book does end, and so does this review—is composed of three chapters. The first, by Larry Marshall, is entitled “Who Killed Cock Robin,” and addresses terminology and what is meant by it all. It is an excellent theoretical and organizational overview. “Explaining Pleistocene Extinction,” by Donald Grayson, is also an excellent historical and philosophical review of the entire debate. The final chapter is by Jared Diamond (see his criticisms of cryptozoology in a March, 1985, *Discover* magazine article). Diamond’s 38-page chapter is a look at historic extinctions and their relevance to the debate. It is a wide-ranging, fascinating review of the many factors involved in modern (historical) extinctions.

In conclusion, I recommend *Quaternary Extinctions* to all who are interested in the large animals of prehistory, in the possible causes of their demise, and in what might have been had we humans not come along.

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*Les Félines-Mystère: Sur les Traces d'un Mythe Moderne* [The Mystery Felines: On the Track of a Modern Myth]. By Jean-Louis Brodu and Michel Meurger. Pagonip (B.P. 195), Paris, 1984. 36 pp. 25 FF. (p.).

The cover of *Les Félines-Mystère* depicts a she-wolf suckling three rather Martian-looking infants. The camera hanging from the she-wolf’s long tail suggests that the triplets (the latter representing perhaps the French republican trilogy: Liberty, Equality, Fraternity) feed on some form of mythology as reflected by the mass media.

This study deals mainly with the so-called Beast of Noth, whose presence was reported, at the end of 1982, in the Creuse region of France. The authors conducted an on-the-spot investigation, interviewing “several witnesses, a few members of the Gendarmerie, some local radio people, and many others, both from the town and the country . . .” Most of the subsequent research was done at the National Library, in Paris.

According to the work’s English abstract, “Jean-Louis Brodu covered the journalistic saga of the ‘Beast of Noth’ as it was told in the two rival daily newspapers of Limoges. Michel Meurger uncovered the sociological, historical, and mythical mechanisms of the rumor process at work in the making of what was to be remembered as ‘Babette, the Beast of Noth’: a truly genuine modern myth in herself.”

The authors begin their study by asking a question: What became of yesterday’s singular fear of werewolves and the like, beliefs which fed on the widespread wolf scare? Did those rural myths, which suggested that the gap between animal and man was never to be closed, disappear at all? The answer is negative if one considers the numerous rumors about the Beast of Vosges in France during the 1970’s, or the phantom felines reported more recently in Britain (the Surrey puma, the Nottingham lion, and the Beast of Exmoor). In May, 1983, the *Daily Express* even offered a £1,000 reward for a picture of the Beast of Exmoor, which had devoured hundreds of livestock. Confronted with such mysteries, writers have proposed various hypotheses, which may generally be divided into two categories. Some, such as Di Francis, assert that an unknown species of native felid exists in Britain. Others affirm that no such animals actually exist, and they provide neo-occultist theories to explain the reports.

The authors of the present work follow a totally different approach. To them, just like in phantom hitchhiker and close encounter UFO cases, what is at stake is neither an animal nor a physical phenomenon, but *people* who express themselves at a crucial period of their lives using the medium of modernized imagery.

In order to solve the enigma of the mystery felines, Brodu and Meurger investigated how the eyewitnesses were side-stepped, their testimonies mod-



ified or dismissed by experts of all kinds, the journalists, the zoologists, and the blackmailers. Even if they admitted the existence of a super German shepherd dog, a giant wild cat, or an authentic large felid, the authors consider that the track followed by the Beast closely resembles the route taken by rumor.

While being interviewed, it became obvious that, in several instances, the various actors in the saga were expressing their *anguish* over the deep changes that are affecting the rural areas in which they live. The Beast thus becomes the concrete symbol of rural fear and social tension.

Besides, it seems that, from time immemorial, the peasants of the center of France have been prone to believe in strange beasts. Novelist George Sand, writing on these beliefs in 1858, described a kind of dog as big as a cow, a composite of various animals. Clear-sighted people would describe it as a greyhound bitch, which, according to Sand, was what it really was. Finally, in their preliminaries, Brodu and Meurger insist on the fact that the people they met were neither hysterical nor schizophrenic.

The core of *Les Félines-Mystère* is really like a detailed case-study. The Beast appears in the village of Noth in the Creuse Department (in the center of France), at the end of 1982. Nicknamed Babette, it is said to look like a lion the size of a calf, and to slay cattle. Attempts to catch it are unsuccessful. So says the daily *Le Parisien* of December 3, 1982.

Apparently, public opinion has been conditioned to the coming of the Beast. The authors underline the fact that, in early November, dinosaur bones had been discovered in the Aude Valley. At about the same time, the motion picture *Cat People*, directed by P. Schrader (a remake of the 1942 J. Tourneur classic movie), was released. In it, a mysterious feline roams about the town, and beautiful Irena, the heroine, discovers that she, herself, is a panther-woman . . . .

The Beast of Noth also appears during a season characterized by extremely bad weather, the wind sometimes blowing at speeds of up to 120 kilometers per hour, the falling of trees killing several people. Thousands of roofs are damaged. Two-thirds of those in the Limousin region are deprived of electricity.

On November 10, a plaster cast of an animal footprint is made near the farm where an 800-pound cow has been killed. Police compare the cast with footprints found in the village of Thouran, 40 kilometers away. The cast and prints do not match. On November 16, two days after another violent storm, a troop of runaway dogs attacks a herd of sheep. One sheep is slaughtered.

On November 21, a Mr. Camille, a farmer, sees the Beast. A search is launched, involving 30 policemen, 11 park wardens, about 20 hunters, and dogs. The daily *Populaire du Centre* of November 24, 1982, publishes a

historical survey, citing reports from as far back as 1698. One author, in 1885, mentions possible links between European werewolves and cases of lycanthropy in Africa.

In December, 1982, the mayor of Noth has casts of the Beast's footprints examined by Dr. Michel Klein, in Paris. His conclusion: the Beast is probably a puma or a young lion weighing about 80 kilos. In January, 1983, Brodu and Meurger visit Mr. Camille, in the lonely, neighboring village of Auzillac (15 inhabitants). He declares: "She [the Beast] turned up because of my dogs. They are all males."

Analyzing the Beast of Noth phenomenon, the authors state that the Beast shares one basic feature with the Sea Serpent and Yeti: it has been seen but never captured. In fact, the existence of the Beast is founded on the *absence* of sign—the mark of a common predator. Stray dogs will devour several sheep, while the Beast kills one animal at a time, one time eating the belly, another time a shoulder. Then, the tracks are atypical, different from those of a canid or a felid.

Whenever it was observed, the Beast was never clearly seen, due to poor weather conditions (fog, rain) or darkness of night. However, descriptions of the Beast follow a well-known *pattern*: it is huge, ill-defined, ubiquitous, and invulnerable. While the moral of the story may change, the pattern of the Beast's whereabouts remains the same throughout the centuries.

In the case of the Beast of Noth, the local folk came to suspect the ecologists (people from urban centers, especially Paris) of having secretly reintroduced lynxes into the countryside. Such a suspicion reflects the native fear of a return to savagery.

Brodu and Meurger discuss numerous UFO incidents in that part of France. Just like the Beast of Noth, they fulfill a social function; they embody, if one may say so, the uneasiness felt by the local folk as the villages are abandoned, the railroads closed, and the forest paths invaded by vegetation. The inhabitants of the Limousin region often warn that "the wolves will soon be back," meaning a return to a form of chaos devoid of humanity.

Any journalist influenced by sociology will keep repeating that "the times they are a-changing." Brodu and Meurger's view of the changes that are affecting French society ring like any other journalist's comment on the subject. In that respect, they might very well have written the following phrases, taken from an article on pit-bull fights published in the weekly *L'Événement du Jeudi* on November 17, 1986: "Nobody would ask the Beast of Gévaudan to look after sheep. When this happens, things begin to deteriorate."

Hardly original, either, is the fact that, following a long tradition, people dump onto the devilish wolf, or a similar beast, their own defects, vices, baseness, cowardice, cruelty, stupidity, gluttony, and savagery. Unpredict-

able and anarchic, the Beast is a symbol of the "fringe." This tradition has been alive, especially in the center of France. In the Limousin region, for instance, the "wolf encloser" was the person who had the strange power of keeping the wolves within a restricted area or enclave, hence the term *enclaveur de loups*. The misdeeds of those "wolf-leaders" are mentioned in issues of *Le Petit Journal* of 1879. The sound historical survey *Les Loups en France* (Claude-Catherine and Gilles Rogache, 1981, Aubler, Paris) will provide the reader with further information on the subject, and, incidentally, will probably convince him or her that the Beast of Noth is simply another noteworthy episode in a long-lasting tradition.

If the Beast symbolizes rural dissatisfaction, its daily behavior is more difficult to explain. One local reporter offers a Freudian interpretation. After having wandered about, the Beast, on its return route, usually follows the banks of either the River Semme or the River Gartempe. The Beast is then torn between two choices: which river to choose? Somewhat oddly, the reporter concludes: "Such an attitude seems to be dictated by maternal instinct."

At this stage, speaking of rivers, why not add that the ancient name of the Tiber was *rumo* (or *rumon*)? The names Rome, Remus, and Romulus are closely linked with the nouns *rio* and *rumor* (running water and running noise). The rivers Semme and Gartempe overflowed in the winter of 1982. Similarly, rumors are spread around during certain periods of history in given areas.

The sociologist is quite justified in studying those "ethnorumors"—a term slightly different from Greenwell's "ethnoknowledge" or "ethnoknown"—since they imply that the so-called Beast has no genuine existence, but is a human creation, a figment of human imagination. However, Brodu and Meurger's explanation remains unsatisfactory. At the sociological level, their answer is much too simple: the countryside has been deserted—a phenomenon which is not new in itself—hence the resulting *anguish* which took the shape of the Beast. They never ask: does the Beast appear periodically? This notion of *periodicity* might eventually shed a novel light on those confusing rumors.

From a journalistic point of view—not to mention an ethnological one—it is hard to expect much novelty from a few on-the-spot interviews conducted during a 2-day stay. Finally, much could be said about Meurger's claim that "he uncovered the mythical mechanisms of the rumor process at work . . . ." As a matter of fact, the book's subtitle, "On the Track of a Modern Myth"—as well as Rome's she-wolf which illustrates the cover—implies that *Les Félines-Mystère* deals mainly with mythology.

Meurger's "radically new approach" will only fool those who have not read Carl G. Jung's *Un Mythe Moderne* (Gallimard, Paris, 1961; *Flying*

*Saucers: A Modern Myth of Things Seen in the Sky*, Harcourt, Brace & World, 1959), in which the physical existence of UFOs remains secondary. The emphasis is on the imaginative power at work within the subconscious, with the result that UFOs are not so much "seen" as "dreamt."

Meurger chose the very same approach with the Beast of Noth without acknowledging it. Jung's book is not even mentioned in his short bibliography. Moreover, he even warns us, in very vague terms, *against* the validity of Jung's theories! This amounts to a display of intellectual dishonesty.

The wealth and depth of Jung's theories have not yet been fully appreciated. Before condemning them, I would advise researchers to read *Un Mythe Moderne*, then to delve into Jung's impressive work *Psychologie et Alchimie* (Buchet-Chastel, Paris, 1970), especially Chapter 4, entitled "Prima Materia." There, one will find the truly enlightening clues needed in order to decipher the modern myth of the Beast of Noth.

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*Curious Encounters: Phantom Trains, Spooky Spots and Other Mysterious Wonders*. By Loren Coleman. Faber & Faber, Winchester, Massachusetts, 1985. 166 pp. \$11.95 (p.).

The frontispiece of Coleman's latest book is, appropriately enough, an illustration of that iconoclast, writer, humorist and collector of all manner of unexplained oddities, Charles Fort. This is truly a Fortean book, filled with an assortment of enigmas guaranteed to stimulate the brain cells of armchair explorers and anyone else willing to admit that there may still be some unknowns and unknowables lurking in the shadows of this vast American continent.

Ranging from sea serpents to phantom trains, thunderbirds, stone forts, and scaly monsters, the subjects covered may at first seem unrelated. However, Coleman is at some pains to show that there is what appears to be an underlying pattern to the strangeness, perhaps associated with time, and perhaps associated with the Earth itself. This is the basic stuff that Fortean delight in: material that is often humorous, always stretching our powers of belief, but with an undercurrent of truth that cries out for serious investigation. This book is no amateurish rehash of yellowing and unsubstantiated



newspaper clips. Coleman has done his fieldwork, and he has interviewed witnesses of many of these curious encounters. In doing so, he challenges those who imagine that real adventure is only to be found in chasing dragons in Africa, or shadowy man-like apes in the vastness of the Himalayas. There are actually both cryptozoological and other mysteries to be found all across the United States.

The initial chapter, "Mysteries from the Depths," includes a gripping eyewitness account of the Casco Bay sea monster from 1958, and a review of some of the enormous squids (*architeuthis*) that have washed ashore in recent times. This is followed by a discussion of the provenance of the incredible sea monster/machine sighted in Puget Sound in 1893, and "flying serpents" seen in various parts of the United States in the mid-nineteenth century. These events clearly defy scientific scrutiny.

Coleman traces the sightings of enormous birds—some the size of small airplanes—from Indian legends, and tales of the early explorers right up to present-day Illinois, Texas, and Pennsylvania. He theorizes that these huge, aggressive birds still exist in hidden areas of the Ohio River Valley, from the Appalachians to the Ozarks. Bearing some similarities to the Andean condor (*Vultur gryphus*) and the Californian condor (*Gymnogyps californianus*), these huge birds have been seen throughout the central states. Coleman theorizes that they may be some form of teratorn.

Coleman then journeyed to Mexico to research the rarely reported—yet apparently real—little people of the Yucatan, the *alux*. There is much circumstantial evidence that these little people exist (or did exist) throughout the area. "Creatures from the Black Lagoon" discusses bizarre sightings from several states of scaly, man-sized, giant "lizards." The chapter on "Phantom Trains" is considerably strengthened by the presentation of material on "ghost lights" gathered by Bob Jones of Vestigia. He and his crew employed an abandoned stretch of "haunted" railway line in New Jersey, sending an instrumented rail car up and down the line under remote control, gathering air samples, measuring humidity, temperature, electrostatic potential, and exposing infrared film. He has shown some striking correlations between "spook lights" and geologic faults, suggesting that additional work might discover a mechanism for the prediction of seismic events.

Enigmatic stone forts and walls exist all over the United States, and Coleman discusses his visits to one on Fort Mountain, Georgia, and to the remains of huge stone walls in the San Francisco Bay area, some as close to the city as Berkeley and Oakland. We can only hope, with Coleman, that these remains of what could be the oldest fortifications in America do not end up under supermarket parking lots.

In the final chapter, he attempts to find some pattern in this crazy quilt of strange phenomena. He examines the striking and readily verifiable phe-

nomenon of similar names being associated with widely dispersed events. He examines the evidence for patterns and cycles in time, and brings together an astonishing number of strange events that occurred on June 24. Of course, as Coleman admits, there can be no real answer. And when we get close to what we think is the answer, we often find ourselves back where we started. As Charles Fort so succinctly put it, "One measures a circle, beginning anywhere."

The book concludes with a forty-page listing of "Phantom Trains, Spooky Sites and Mystery Spots" in North America. Some of them are sure to be in the reader's area, and might well be worth a visit. Coleman is to be congratulated for his humorous, critical, and rational approach to a subject matter that is most often used as space filler in newspapers at Halloween time. Something is out there. That's for sure.

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## Comments and Responses

*This section permits readers to critique or comment on works previously published in Cryptozoology. The original authors and other readers are encouraged to respond to these critiques or comments. Readers are also encouraged to critique or comment on the works appearing in this issue. All comments are the responsibility of the authors only, and do not reflect any policies established by the Editor or the Editorial Board of Cryptozoology, or the Board of Directors of the Society.*

### CHECKING THE CHECKLIST

(Comment on Heuvelmans, *Cryptozoology*, Vol. 5: 1–26)

In his annotated cryptozoology checklist, Bernard Heuvelmans has reviewed an extremely large body of literature. For this alone, publication of the checklist is noteworthy. It is unfortunate, but understandable because of its length, that complete literature citations could not have been included. This omission earmarks the checklist as an article for laymen, rather than as a paper for professional zoologists. However, if it encourages laymen to read *Cryptozoology*, then this is a bonus for the Society.

The subdivision of the checklist by taxonomic and geographic classifications adds to its professional utility, and raises several points for comment:

1). I agree whole-heartedly with Heuvelmans' decision to include all such supposed animals with sufficient documentation, even though some of these may "be based upon misidentifications, exaggerations, or hoaxes." They should all be of concern to cryptozoologists, and *disproving* the existence of a supposed animal should carry just as much merit as *proving* the existence of one.

2). While I am pragmatic enough to understand why it is done, in principle I do not think we should limit our study of cryptozoology to "large-to-medium-sized animals." If a small species meets the other criteria, then it should be included. Also, although Heuvelmans claims to have coined the term "cryptozoology" (*Cryptozoology*, Vol. 3: 11), Theodore Savory (*Scientific American*, Vol. 219[1]: 108) states that the name "cryptozoa" was originally suggested by Arthur Dendy in 1895 to describe seldom seen animals, such as soil invertebrates. Although the connotation has changed in

present usage, I see no reason to impose size restrictions on our definition of "cryptozoology."

3). Heuvelmans suggests that his checklist is incomplete because it is the work primarily of only one person. He does not give himself enough credit. I know of no evidence to suggest that a more complete job could have been done—in the limited space available—by two, or even 200 people.

4). Another reason for suggesting that this checklist is only a start is that poorly documented animals, and well known species that have become rare, have not been included. While I generally think that this is the proper approach, I believe that animals should not be omitted simply because they are "on the verge of extinction" if they meet the other criteria for cryptozoological study. If one purpose of the checklist is to encourage the search for these animals, those thought to be close to extinction should perhaps receive the highest priority.

5). It is most interesting to note that almost 75 percent of the animals in the checklist are thought to be mammals. Of all known animal species, mammals constitute only about one-fourth of one percent—the majority of all animal species being, of course, invertebrates. One would thus expect that, of all *unknown* animals, mammals would constitute no more than this same percentage. Therefore, this large discrepancy must be related to one of the following factors:

- It is the *larger* unknown animals which are noted, reported, and recorded in the cryptozoological literature (and this would be in accordance with a fundamental premise of cryptozoology: that unknown animals must be large enough to be noted and remembered by observers);
- there is a tendency to classify unknown animals with the most familiar groups;
- "higher" unknown animals are more skilled at avoiding capture;
- mammals have lower minimum population sizes;
- individuals spreading myths have a limited imagination;
- some combination of causes.

In conclusion, I would like to emphasize that, overall, Heuvelmans' new checklist should prove useful to all those involved in the study of cryptozoology.

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## THE LINNAEUS OF THE ZOOLOGY OF TOMORROW

(Comment on Heuvelmans, *Cryptozoology*, Vol. 5: 1–26)

Here, at last, is Bernard Heuvelmans' checklist summarizing 40 years of research, a synthesis which every cryptozoologist has long been waiting for. This is particularly true outside of the French-speaking countries, where several of Heuvelmans' books are unavailable, as they still need to be translated into English. The present article is, as usual, brilliant, and Heuvelmans must be considered, more than ever, the Linnaeus of the zoology of the twenty-first century.

All the same, I would like to add some complementary data, and to draw some conclusions. First, the total number of still unknown forms (110 to 138) may appear to some to be very high, particularly when certain closed-minded zoologists still think that only very small animals remain to be scientifically discovered: there is overwhelming evidence to the contrary; to wit, the last 5 years alone have provided a rich "harvest" of new species of large animals.

Among aquatic mammals, a new species of killer whale (*Orcinus glacialis*) from the southern polar seas has been described by two Soviet mammalogists (A. A. Berzin and V. L. Vladimirov, 1983, A New Species of Killer Whale [Cetacea, Delphinidae] from the Antarctic Waters [in Russian], *Zoologicheskii Zhurnal*, Vol. 62[2]: 287–95), and, on land, a new gazelle (*Gazella bilkis*) from North Yemen has been described (Colin P. Groves and Douglas M. Lay, 1985, A New Species of the Genus *Gazella* [Mammalia: Artiodactyla: Bovidae] from the Arabian Peninsula, *Mammalia*, Vol. 49[1]: 27–36).

Among birds, a new species of albatross (*Diomedea amsterdamensis*), with a wingspan of 3 meters, has been described from Amsterdam Island, in the southern Indian Ocean (P. Jouventin and J. P. Roux, 1983, Discovery of a New Albatross, *Nature*, Vol. 305: 181).

Among reptiles, 1986 was the year of the giant gecko, with the description of a specimen killed in the Iranian Khuzistan during the Iran/Iraq war (M. Baloutch and M. Thireau, 1986, Une Espèce Nouvelle de Gecko *Eublepharis ensafi* [Sauria, Gekkonidae, Eublepharinae] du Khouzistan [Sud Ouest de l'Iran], *Bulletin Mensuel de la Société Linnéenne de Lyon*, Vol. 55[8]: 281–88). This lizard is 40 centimeters long, whereas the largest previously known geckos, like the tokay (*Gekko gekko*), seldom exceed 30 centimeters in length.

Coincidentally, another new species of a still larger gecko was described almost simultaneously from a specimen found, strangely enough, in the collections of the Museum of Natural History of Marseilles—which reminds us of similar cryptozoological precedents, such as the pygmy chimpanzee and the Congo peacock, both found also in a museum. This truly monstrous gecko is 62 centimeters long, twice the length of the tokay. Its origin is not known for certain, but the specimen was most probably brought back from New Zealand about 150 years ago, possibly by the French explorer Dumont

d'Urville (Aaron M. Bauer and Anthony P. Russell, 1986, *Hoplodactylus delcourti* n. sp. [Reptilia, Gekkonidae], the Largest Known Gecko, *New Zealand Journal of Zoology*, Vol. 13[1]: 141–48).

What is significant from the cryptozoological point of view is that "huge saurians" variously called *taniwha*, *moko*, *ngarara* or *kawekaweau* by the Maoris have been reported a number of times from New Zealand over the last two centuries (H. D. Skinner, 1964, Crocodile and Lizard in New Zealand Myth and Material Culture, *Records of the Otago Museum*, Vol. 1: 1–43). At least the *ngarara* and the *kawekaweau* have been described as gecko-like: there is even a story about a captive *ngarara* fed with roots and small birds (geckos of the genus *Hoplodactylus* from New Zealand happen to be omnivorous), and which emitted guttural sounds, typical of these lizards (J. W. Stack, 1875, On the Disappearance of the Larger Kinds of Lizard from Northern Canterbury, *Transactions of the New Zealand Institute*, Vol. 7: 295–97).

As Bauer and Russell conclude, "it is possible that a 370 mm SVL [snout-vent length] gecko with nocturnal habits, isolated and hidden retreats, and low population density, might still go unnoticed." The name *taniwha*, on the other hand, appears to be as vague as our own "monster" or "dragon," but might be partly based, in my opinion, on the memory of large monitor lizards, now probably extinct, an opinion also expressed by other cryptozoologists (Roy P. Mackal, *Searching for Hidden Animals*, 1980, Garden City, New York, Doubleday: 122).

Among fishes, the most extraordinary discovery of recent years is, of course, that of the megamouth shark in 1976, a plankton feeder 4.5 meters long, only described scientifically 7 years later (R. Leighton Taylor, L. J. V. Compagno, and Paul J. Struhsaker, 1983, Megamouth—A New Species, Genus, and Family of Lamnoid Shark [*Megachasma pelagios*, Family Megachasmidae] from the Hawaiian Islands, *Proceedings of the California Academy of Sciences*, Vol. 43[8]: 87–110).

And, last but not least, among cephalopods, a large cirrate octopod 2.5 meters long was filmed in March, 1984, over a hydrothermal spring in the northeastern Pacific from the French diving saucer *Cyana*—a species which still awaits a scientific name. A description will probably be given by Gilbert L. Voss, in Volume 12 of *The Mollusca* (Karl M. Wilbur [ed.], New York, Academic Press), to be published soon.

All these "unexpected" animals belong to the same classes as the "unknown" animals already listed by Heuvelmans, who states that his checklist is neither restrictive nor fixed. Indeed, apart from the New Zealand saurians—at least the giant gecko, the existence of which is based essentially on sightings—which he curiously overlooked (this species *partly* shifted, in the meantime, to a zoological status), two marine animals should be added to his checklist: oversized great white sharks, and a large mysticete whale.

Oversized great white sharks (*Carcharodon* sp.), far exceeding the 6 to 9

meters in length of the largest specimens of *C. carcharias*, have been reported mainly from the seas off Australia and South Africa, and huge *Carcharodon* teeth have been dragged from the bottom of the oceans (David G. Stead, 1963, *Sharks and Rays of Australian Seas*, Melbourne, Sydney: 38–47). As some teeth of *C. megalodon*, the so-called prehistoric great white shark, have turned out to be only 10,000 years old (Wladimir Tschernezky, 1959, Size of *Carcharodon megalodon*, *Nature*, Vol. 184[4695]: 1331–32), some authors have even speculated on its possible survival to the present (J. L. B. Smith, 1965, *The Sea Fishes of Southern Africa*, Cape Town, Central News Agency: 49).

Though all unknown cetaceans listed by Heuvelmans are either archeocetes or odontocetes (toothed whales), at least one large mysticete remains to be discovered: this form, mainly characterized by the presence of two dorsal fins, was observed off Peru in 1867 by Giglioli, an Italian naturalist, during the *Magenta* expedition around the world. He gave an excellent description of this cetacean, and proposed for it the scientific name of *Amphiptera pacifica* (Enrico Hillyer Giglioli, 1870, *Note Intorno alla Distribuzione della Fauna Vertebrata nell' Oceano Prese Durante un Viaggio Intorno al Globo 1865–68*, Firenze, Giuseppe Civelli: 75–76).

A common misconception, now invalidated by Heuvelmans' checklist, is that the animals with which cryptozoology is concerned are "living fossils"; that is, animals supposedly extinct since geological times still surviving today. However, the forms which *might* be so-called "prehistoric animals" actually account for less than 20 percent of the total, and the other unknown forms—more than 80 percent!—belong to groups well represented today. Among the aquatic forms, these are pinnipeds, sirenians, cetaceans, lizards, crocodilians, snakes, sharks, eels, catfishes, squids, and octopuses; only three or four forms belong to the supposedly extinct suborder of archeocetes, and only six could be dinosaurs, plesiosaurs, or mosasaurs. Among the terrestrial forms, we are simply dealing with ungulates, ursids, felids, canids, primates, bats, a rodent (or hyracoid?), a few birds, a glass snake, and some true snakes, all belonging to present families: only some flying forms could possibly be pterosaurs.

Another common misconception, now also proved to be wrong, is that the hidden creatures in question are necessarily huge. While Sasquatch would be the largest of all the primates, the monitor lizards of Australasia the largest of the Varanidae, the giant octopus the largest of the Octopoda, etc., the pygmy elephant, the forest rhinoceros, the hyrax-like animal from Ethiopia, and even the Yeti—among many others—appear rather small within their groups. What can be said, as Heuvelmans has emphasized elsewhere, is that cryptozoology deals with relatively large animals, of a "sufficient" size, otherwise the unknown animal might not be recognized as such by the layman; in fact, it would not even be noticed. In Heuvelmans' checklist, about 90 percent of the animals weigh more than about 50 pounds (23 kg),

but 10 percent of them weigh less, which is not to be ignored. To sum up, cryptozoology seems to be largely restricted to vertebrates and to a few invertebrates (mainly mollusks), and is not concerned with insects, mites, or protozoans. But this is not a strict rule.

So, when some authors claim that cryptozoologists are only after "large and mostly monstrous" animals (Alvin Novick, 1981, *Beyond Bigfoot*, *BioScience*, Vol. 31[10]: 775), or "largely vertebrate, and preferably human-like or dinosaur-like" animals (Robert M. May, 1984, *Cryptozoology*, *Nature*, Vol. 307: 687), or "preferably primates, carnivores or long-extinct taxa such as dinosaurs" (Jared M. Diamond, 1985, *How Many Unknown Species Are Yet To Be Discovered?*, *Nature*, Vol. 315[6020]: 538–39), they are completely wrong.

The proportion of some zoological groups in Heuvelmans' checklist may appear to be abnormally high (primates, carnivores, etc.), or abnormally low (birds), compared to the total number of known species in these groups. Can this be explained?

In order to answer this question, one must wonder why these animals still remain elusive and undiscovered. The first answer which comes to mind is, of course, because they remain well hidden, which justifies the very name of cryptozoology (the science of hidden animals), created by Heuvelmans for this field of research: they are hidden in the depths of the oceans, murky lakes and other inland waters (all sea and freshwater "monsters"), in high mountain ranges (*tatzelwurm*), in large temperate forests (Sasquatch), in swampy jungles (Mokele-Mbembe), or even in underground habitats (*minhocao*); in fact, everywhere man cannot penetrate easily.

This explains also why there are only 40 to 52 unknown aquatic forms against 70 to 85 terrestrial ones. Such a result is quite consistent with the history of zoological discoveries: almost all (if not all) the large terrestrial animals were already known to the natives: among the animals described during this century, the okapi (*Okapia johnstoni*) was known as *o-api* to the Wambuti Pygmies; the great forest hog (*Hylochoerus meinertzhageni*) as *elquia* to the Masai, *tumtu* to the Nandi, and *mbirri* to the Kakumega; the Komodo dragon (*Varanus komodoensis*) as *boeaja darat* to the Komodo and Flores Islanders; the Congo peacock (*Afropavo congensis*) as *itundu* to the Bakumu, and *ngowe* to the Wa-Bali; the kouprey (*Bos sauveli*) as *kouproh* to the natives of Cambodia; and the Chacoan peccary (*Catagonus wagneri*) as *tagua* to the inhabitants of Paraguay's Gran Chaco.

On the other hand, few large *aquatic* animals described since 1901 were already known to native peoples: I can only recall the Tung Ting Lake dolphin (*Lipotes vexillifer*) known as *pei chi* to the Chinese, and the coelacanth fish known as *kombessa* to the Comoros fishermen. That is what one would expect with aquatic animals, which are, of course, seldom observed, well hidden as they are in marine and freshwater depths.

A zoogeographical review of the results given by Heuvelmans is also very



interesting: there is a striking penury of unknown forms from the Nearctic and Palearctic regions, though the latter is the largest of all the zoogeographical regions. Both of these regions are actually considered the best known ones, zoologically speaking, and few large animals have been described in them since 1901. These regions are easier to penetrate and study, and they are also, incidentally, where the discipline of zoology was born and developed. The other regions were geographically unknown to Western science not so many centuries ago, and they are still difficult to explore (particularly the tropical jungles). Consequently, they are, at the very least, poorly-known to zoologists. These regions are precisely those which produced the most remarkable zoological discoveries mentioned above, from the okapi to the Chacoan peccary. If cryptozoology were not based on something real, how could one explain this (relative!) want of a cryptofauna in North America, northern Asia, and Europe?

Not only are still unknown animals *passively* hidden by the nature of their environment, but they often hide themselves *actively*: they can do so because they are fast runners (*onza*, *koao*—the Marquisian bird—etc.), fast swimmers (dolphins and pinnipeds), burrowing animals (*minhocao*), or nocturnal (wild men of northern Asia), and, moreover, they cunningly shun humans when they are not just naturally shy and wary. Interestingly, it may be observed that the zoological groups which are the richest in the number of unknown animals are those endowed with the highest intelligence: primates (and, above all, hominoids/hominids), felids, cetaceans—and even cephalopods, by far the most intelligent forms among the invertebrates (M. J. Wells, 1962, *Brain and Behaviour in Cephalopods*, London, Heinemann), though, in this case, their marine habitat is in itself sufficient to explain their *incognito*. This accounts well for the fact that, among the 57 to 68 terrestrial mammals, 17 to 24 are primates, a very high proportion indeed (about 30 percent), and 10 are felids (about 15 percent). In the case of primates, a psychological factor is also instrumental: as they are the animals which most resemble man, they are more likely to appear abnormal, and thus to be mythified, sensationalized, and more often reported. The partiality of certain cryptozoologists for wild hominids (and for them only) is nothing but a scientific expression of this same bias. This, eventually, increases the proportion of *writings* about unknown primates, which is what led the late George Gaylord Simpson to state that, “among mammals cryptozoological search is largely for supposed primates” (George Gaylord Simpson, 1984, *Mammals and Cryptozoology, Proceedings of the American Philosophical Society*, Vol. 128[1]: 1–19)—a complete misunderstanding of the whole problem, if not sheer nonsense.

There are only three birds listed by Heuvelmans, as should be expected: the “father” of cryptozoology has shown in a previous article that the ornithological inventory of the world is almost complete (which is easy to

understand, for these flying animals are particularly conspicuous), and that an average of only three species of birds are now described per year (Bernard Heuvelmans, 1983, *How Many Animal Species Remain To Be Discovered?*, *Cryptozoology*, Vol. 2: 1–24). Though Heuvelmans' estimate has been criticized by some, a careful species by species review of recent ornithological discoveries demonstrates that an average of 2.5 “good species” have been described per year for the period 1941–55, 3.5 for 1956–65, 2.8 for 1966–75, and possibly 2.9 for 1976–82 (Ernst Mayr and François Vuilleumier, 1983, *New Species of Birds Described from 1966 to 1975, Journal für Ornithologie*, Vol. 124[3]: 217–32). Of these three unknown birds, two are flightless, thus more prone to remain hidden.

Reptiles are numerous, about 20 percent of the total number, and are mainly monitor lizards (which often have aquatic habits). Incidentally, they are spread over several regions (Indian subcontinent, Malaysia, Australia, New Guinea—and maybe New Zealand, as discussed above), which once formed Gondwanaland, subsequently broken up into various land masses through continental drift. This provides further support for the possible existence of these numerous unknown varanids.

The only surprise in Heuvelmans' checklist is perhaps the total absence of amphibians, a group now admittedly restricted to rather small animals. On the whole, however, the results are quite logical and consistent, and they provide, in fact, more circumstantial evidence of the soundness of cryptozoology itself.

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#### A REEVALUATION OF SOME CRYPTOZOOLOGICAL ANIMALS

(Comment on Heuvelmans, *Cryptozoology*, Vol. 5: 1–26)

Bernard Heuvelmans' annotated checklist of apparently unknown animals provides an excellent summary of known cryptozoological examples. It is not my intention here to discredit this list in any way, but to provide some comments on a few of the examples, resulting primarily from an update in our knowledge about the fossil record in the past decade. My experience is solely with terrestrial, non-primate mammals, and these comments, with the exception of the comment on the giant monitor lizard, is limited to such groups.

The giant fossil monitor lizard *Varanus priscus* (p. 11) is actually known

as *Megalanias prisca* (Max K. Hecht, 1975, The Morphology and Relationships of the Largest Known Terrestrial Lizard, *Megalanias prisca* Owen, from the Pleistocene of Australia, *Proceedings of the Royal Society of Victoria*, Vol. 87: 239–249). The Pleistocene fossils of this animal represent a lizard of very large size indeed, possibly weighing as much as 500 kg (the size of a large cow).

I was surprised to see that Heuvelmans did not mention the possibility of the East African Nandi Bear (p. 18) being a surviving chalicotherid, as that was the opinion expressed in his earlier works (e.g., Bernard Heuvelmans, 1958, *On the Track of Unknown Animals*, Rupert Hart-Davis, London), and is a view that has been expressed by a number of paleontologists as well (C. W. Andrews, 1923, An African Chalicotherid, *Nature*, Vol. 112[2819]: 696; L. S. B. Leakey, 1935, Does the Chalicotherid, Contemporary of the Okapi, Still Survive?, *The Illustrated London News*, Vol. 187[5037]: 730–33, 750; Martin Pickford, 1975, Another African Chalicotherid, *Nature*, Vol. 253: 85; Robert J. G. Savage and Michael R. Long, 1986, *Mammal Evolution: An Illustrated Guide*, British Museum [Natural History], London). Evidently, Heuvelmans has changed his mind about the most probable identity of the Nandi Bear, but his earlier view is still maintained by a number of people, and is one that I personally favor.

The “small hyrax or marmot” of Southern Ethiopia (p. 20) is almost certainly a hyrax. Marmots are found today only in North America and Eurasia, and have never been present on the African continent. In contrast, hyraxes originated in Africa, and the number of separate species is still in dispute. Being found in the arid Ethiopian regions, this would probably represent an unknown species of *Procavia* (rock hyrax) or *Heterohyrax* (bush hyrax).

The southern Ethiopian deer (p. 20) is extremely unlikely to represent a specimen of *Climacoceras* (termed “*Climatoceras*” by Heuvelmans). Although *Climacoceras* has branched antler-like cranial appendages, and was originally thought to be a deer (D. G. MacInnes, 1936, A New Genus of Fossil Deer from the Miocene of Africa, *Zoological Journal of the Linnean Society*, London, Vol. 39: 521–30), it is, in fact, a primitive relative of the giraffes (W. Roger Hamilton, 1978a, Fossil Giraffes from the Miocene of Africa, and a Revision of the Phylogeny of the Giraffoidea, *Philosophical Transactions of the Royal Society [London]*, Series B, Vol. 282: 165–229). Moreover, *Climacoceras* was found in East Africa from the late early Miocene (18 million years B.P.) until the end of the earliest late Miocene (6 million years B.P.), but is unknown from later deposits. The possibility of it surviving in a forest habitat—and therefore not being preserved—is unlikely, as the form of the teeth and the proportion of the limbs suggest it was a fairly open habitat animal; it probably lost out in competition to the antelope arriving in Africa in the middle Miocene. A much more likely

contender, in my opinion, is a subspecies of the fallow deer, *Dama dama*, that was found in Ethiopia and Egypt from the late Pliocene (2 million years B.P.) to the late Pleistocene (10,000 years B.P.) (W. Roger Hamilton, 1978b, Cervidae and Palaeomerycidae. In V. J. Maglio and H. B. S. Cooke [eds.], *Evolution of African Mammals*, Harvard University Press, Cambridge).

The otter-like mammal (*waitoreke*) of New Zealand (p. 22) is extremely unlikely to be a true otter, as otters are not found in the Australasian region. It could be a kind of platypus, as suggested by Heuvelmans. Another likely candidate could be the Australian water rat, *Hydromys chirogaster*, that belongs to the rodent subfamily Hydromyinae, which is found exclusively in Australia, New Guinea, and the Philippines. The Australian water rat is of a similar size as the platypus, and it is often mistaken for this animal in eastern Australia. It is a semiaquatic freshwater species that parallels the Northern Hemisphere muskrats (subfamily Microtinae). Because New Zealand harbors no endemic mammals, the island must have split off from the Australasian land mass before the first evolution of mammals in the late Triassic, and thus, on biogeographic grounds, there is no preference for the identity of this mammal being a platypus or an otter. New Zealand is rather distant from Australia for a freshwater mammal to arrive there by rafting, and I would assume that, if it exists, the *waitoreke* somehow “hitched a ride” with the early arriving Maoris. If this was the case, then I would imagine that a rodent, having less specialized feet—and a greater degree of manual dexterity—than a platypus, would be the most likely contender.

Heuvelmans considers the “outsized rabbits” of central Australia (p. 23) to have possibly been species of the extinct wombat-like marsupial *Diprotodon*, or of the giant extinct kangaroo “*Palorchestes*.” While *Palorchestes* was originally thought to be a kangaroo, based only on dental evidence, further fossil material has since been uncovered that shows it was clearly not a kangaroo (see below). The “giant kangaroos” that Heuvelmans refers to are members of the extinct kangaroo subfamily Sthenurinae—although a living member of this subfamily may remain in the banded hare-wallaby, *Lagostrophus fasciatus* (Tim F. Flannery, 1983a, Review of the Subfamily Sthenurinae [Marsupialia] and the Relationships of the Species of *Troposodon* and *Lagostrophus*, *Australian Journal of Mammalogy*, Vol. 6: 15–28). The extinct sthenurines appeared to have been open or arid habitat-adapted browsers, and included the Pliocene and early Pleistocene genus *Troposodon*, which was about the size of large living kangaroos, and the larger Pleistocene genera *Sthenurus* and *Procoptodon* (Tim F. Flannery, 1983b, A Gigantic Relative of the Banded Hare Wallaby, and 1983c, Short-Faced Giant Among Kangaroos. In S. Quirk and M. Archer [eds.], *Prehistoric Animals of Australia*, Australian Museum Publications, Sydney; Pat V. Rich and G. F. Van Tets, 1985, *Kadimakara: Extinct Vertebrates of Australia*, Pioneer Design Studios, Melbourne). Sthenurines, in my opinion, are much



more likely contenders for the role of "giant rabbits" than the lumbering *Diprotodon*. The question remains: why would people who were already familiar with other kangaroos confuse these animals with rabbits? One answer may lie in their difference in morphology. Sthenurines had shorter faces, more heavily built forearms, and shorter tails than living kangaroos (subfamily Macropinae), and thus may have appeared more "rabbit-like." As a further speculation, as they appear to have been fairly arid-adapted throughout their evolution (probably becoming extinct in the late Pleistocene because the savanna areas became too arid for their survival), it is conceivable that they had longer ears, as is common in many arid zone rodents and rabbits today.

Heuvelmans suggests that the "large tapir-like marsupial" of Papua New Guinea (p. 23) might be related to the Australian *Diprotodon*. This animal is actually more likely to be *Palorchestes*, which, as stated above, is actually a relative of *Diprotodon* rather than a kangaroo, and has a skull morphology suggesting a tapir-like snout (A. Bartholomai, 1978, The Rostrum in *Palorchestes* Owen [Marsupialia, Diprotodontidae]. Results of the Ray E. Lemley Expeditions. Part 3. *Memoirs of the Queensland Museum*, Vol. 6: 15–28), more extreme than any that might have existed in *Diprotodon*. *Palorchestes* was found from the middle Miocene (14 million years B.P.) to the late Pleistocene. To the best of my knowledge, neither *Diprotodon* nor *Palorchestes* fossils are known from New Guinea, but there was variable land connection between Australia and New Guinea during the Pleistocene (Tim F. Flannery, in press, Dating the Great New Guinea–Australia Vicariance Event: New Evidence for the Age of Australia's Tertiary Mammal Fauna. In S. Turner and A. Thulborn [eds.], *Problems in Vertebrate Biology and Phylogeny: An Australian Perspective*), and it is conceivable that such an animal could have reached the island from Australia.

Finally, I would like to reiterate my original intention in this Comment. That is, it is intended as a supplement to Heuvelmans' excellent compendium of cryptozoological examples, and is not intended as a major critique of the worth of his article as a whole.

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# A BUNGLING IN THE JUNGLE, OR, WHY SPECIALIZATION IS IMPORTANT IN CRYPTOZOOLOGY

(Comment on Heuvelmans, *Cryptozoology*, Vol. 5: 1–26)

I was surprised to see Bernard Heuvelmans' retraction of his old hypothesis that surviving Australian thylacoleonids (marsupial lions) might account for the cryptophenomenon variously known as the Queensland Tiger-Cat, the Queensland Marsupial Tiger, or, more simply, the Queensland Tiger. Heuvelmans' statement (p. 23) that "medium-sized carnivorous marsupials, distinctly striped and thus referred to as pouched 'tigers,' reported mostly from all the western and southeastern states of Australia . . . are much more probably relicts of the supposedly extinct mainland form of the thylacine (*Thylacinus cynocephalus*)" was probably made in reaction to the Western Australia "thylacine" photos published recently in *New Scientist* (1986, Vol. 110: 44–47).

Even in his most in-depth coverage of Australian "tigers" (1958, *On the Track of Unknown Animals*, Hill and Wang, New York: 210–20) Heuvelmans never recognized the dichotomy of characteristics distinguishing coastal Queensland "tiger" sightings from those of other regions, other than that "a shrewd observer would have remarked that whenever a report came from the north of Queensland it was more positive and more precise" (Heuvelmans, 1958, above: 216). One then wonders why his new checklist statement reports "tigers" "mostly from all the western and southeastern states," apparently relegating the Queensland issue to lesser importance, or even to the trash bin.

I think Heuvelmans' mistake was grouping all the "tiger" reports together in the first place. Unfortunately for a uniting "thylacine theory," a cursory examination reveals distinct features in the Queensland reports which do not resemble the thylacine at all. For example, Malcolm Smith excluded most of the Queensland sightings from his consideration of the mainland thylacine survival question (1982, Review of the Thylacine [Marsupialia, Thylacinidae]. In Michael Archer [ed.], *Carnivorous Marsupials*, Royal Zoological Society of New South Wales, Sydney) because almost all referred to an animal with a rounded, cat-like face rather unlike the thylacine's, which is dog-like. This feline resemblance is undoubtedly what led Heuvelmans to propose the "*Thylacoleo* theory" in the first place. Since then, others have echoed his theory (e.g., Peter Makeig and Janeice Plunkett, 1970, Is There a Queensland Marsupial Tiger?, *The North Queensland Naturalist*, Vol. 37[152]: 6–8), but none have provided any new arguments in support of it.

I have recently argued that other reported Queensland Tiger-specific characteristics, such as tusk-like fangs and arboreal capacity, fit in very well with

our most up-to-date knowledge of *Thylacoleo* (Victor A. Albert, The Queensland Tiger: Evidence for the Possible Survival of the Marsupial Lion, *Thylacoleo*, into Recent Times," paper presented at the Symposium *Some Cats of Cryptozoology*, 1987 Membership Meeting, International Society of Cryptozoology, Royal Museum of Scotland, Edinburgh, July 26, 1987). I have also noted that the biogeographical distribution of the reported sightings seems to coincide with patchily distributed rain forest refugia, largely unchanged since Pleistocene times, which might have supported, and might continue to support, native thylacoleonids. Furthermore, this persistence would probably never have shown up in the fossil record, as fossil formation is very poor in rain forests.

Maybe the thylacine does survive in western and southeastern Australia. Based on the sighting descriptions, there is certainly no reason to propose that striped cryptoanimals from these areas are thylacoleonids. The Queensland Tiger is another story. Perhaps it is a short-faced geographic race of the mainland thylacine, or an unknown, striped variety of the usually-spotted native cat (not a cat at all, but a marten-like carnivorous marsupial). In my opinion, parsimony favors *Thylacoleo* as the most likely candidate for the following reasons: (1) the morphology of the reported Queensland Tiger is very compatible with what we know of *Thylacoleo*, and (2) a *Thylacoleo* explanation does not require the invention of a new animal, only the persistence of one supposedly extinct on the mainland for some 16,000 years.

Heuvelmans' original theory and his recent retraction are "lumper" approaches to what appears to be a heterogeneous collection of cryptozoological reports. Though known for moderate cryptotaxonomic "lumping," especially within marine cryptozoology (p. 7), Heuvelmans is usually careful to distinguish confused or improperly "lumped" criptids (for example, see his discussion of the *iemisch-ellengassen* problem in *On the Track of Unknown Animals*, pp. 253–82). Why, then, the "tiger" blunder? Obviously, as Heuvelmans himself recognizes, no one person can specialize effectively in *all* cryptozoological areas. I think that excessive lumping—and other improper handling of data—becomes much more likely the broader the database. Thus, I would encourage all cryptozoologists to specialize.

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# CHECKLIST CORRECTED AND COMPLETED

(Response to Tomasi, Raynal, Janis, and Albert)

My aim in having the cryptozoology checklist published was twofold: first, I thought it would be helpful to all those involved in cryptozoological research, and, second, I hoped it would be properly corrected and completed by some of them. This wish has been granted beyond all my hopes, especially since some of the comments have come precisely from the most competent and best-informed members of our cryptozoological community.

The two basic sources of cryptozoology are bibliography and iconography. Now, I know of no more zealous, scrupulous, and successful bookworm than Michel Raynal; although I had tentatively covered the essentials of bibliographic research on unknown animals during the last 40 years, Raynal still succeeds today in discovering some old references I overlooked, as well as many recent ones, and this considerably enriches the dossiers of cryptozoology, my own in particular. His enlightening Comment, with which there is not much fault to find, allows us to add no less than three or four animal forms to the checklist, and this in itself is remarkable.

Concerning iconography, I venture to state that, by applying her scholarship in mammalogy and paleobiology to a thorough study of intriguing animal representations from the archaeological record, Christine Janis is, in my opinion, bound to play a leading role in the future of cryptozoology. My fondest hope is to see her write someday the much-needed book showing the importance of archaeology in cryptozoological research.

Shifting now from fundamentals to the study of particular cases, my deep esteem goes also to Victor Albert for his in-depth treatment of the Queensland Tiger problem, which greatly clarifies this irritating mystery—certainly one of the finest cryptozoological feats of recent years. But more on this later.

Before I respond seriatim to the various objections or criticisms raised by my commentators, a point should be stressed. I am presently moving my Center for Cryptozoology (please note my new address at the end of this Response), and all my books, files, and index cards are temporarily packed away for several months in cardboard boxes (about 150, totaling more than 5 long tons in weight!). Thus, I am quite incapable at this time of supporting some of my statements below with full and precise references. I loathe this, but, with the publishing deadline involved, what else can I do?

Since Michel Raynal's detailed Comment answers at length Thomas Tomasi's questions about the striking discrepancy between the total number of animal species in any particular group and the number of the latter's representatives in the checklist, there is not much I can add. I have already answered all the other points raised by Tomasi in former issues of this



journal. One example is when he states that the name "cryptozoa" antedates "cryptozoology"—so what?—which presumably should have prevented me from coining the latter. Also, I have never stated that cryptozoology *should* be restricted to "large-to-medium-sized animals," which happen to be the most numerous of its objects of study. On the contrary, I have been the first to stress that animals of *any* size can be of concern to cryptozoology, provided that they are endowed with some striking traits, unusual enough to make them extraordinary, *and thus apt to be reported*. Neither have I ever stated that animal forms have to be omitted just because they are "on the verge of extinction." If some of these were not included in the checklist, it is because they did not meet the other criteria of cryptozoology, which aims at the discovery of *unknown* animal forms, even if these represent only new subspecies.

In reply to Christine Janis's numerous observations, I would like to state the following:

1. Some herpetologists, at least, no longer accept that the giant fossil monitor lizard of Australia belongs to a distinct genus, *Megalanina*, and thus now call it *Varanus priscus*.

2. I never considered the chalicothere "the most probable identity of the Nandi Bear"; I considered it merely one of the many possible explanations for this protean beast, and I still do. But a checklist aimed at brevity and conciseness could hardly review all the relevant hypotheses for each individual entry.

3. I did not speak of a "small hyrax or marmot" of southern Ethiopia, but of a "small-sized animal, resembling a hyrax or a marmot," the latter meaning any very large rodent as bulky as a marmot. The hyrax hypothesis is probably the soundest.

4. "*Climacoceras*" is, of course, a misprint. That *Climacoceras* is a primitive giraffe with branched "antlers" rather than a deer does not change the fact that it looked like a deer, and such a deer-like animal was actually reported a number of times in southern Ethiopia, not in a "forest habitat," but in the "fairly open habitat" where *Climacoceras* probably dwelt. I owe this information (like that on the possibly undescribed "dassie"), and the tentative identification of the deer-like animal, to L. S. B. Leakey, who communicated them to me in 1967 when I visited him in Nairobi, during my field investigations into reports of the Nandi Bear and the spotted lion, among others.

5. Concerning the *waitoreke*, Janis contends that, "because New Zealand contains no endemic mammals" and because of what this implies on the biogeographical level, "there is no preference for the identity of this mammal being a platypus or an otter." Just as when I wrote *On the Track of Unknown Animals* (1958, Rupert Hart-Davis, London; Hill and Wang, New York), I still maintain that, if this otter-like animal exists in New Zealand, and if it

is actually endemic—which is certainly the more likely possibility—then New Zealand *does* contain at least one endemic mammal. Therefore, it is very probable, on geological and biogeographical grounds, that it is, like the very similar-looking platypus, a representative of the pre-marsupial order Monotremata. My second choice would be that it is a marsupial otter, such as the Central American yapock, but it seems very unlikely to be a placental mammal introduced, even inadvertently, by the Maoris.

6. I completely agree with Janis that the "outsized rabbits" of central Australia are much more probably related to *Sthenurus* or *Procoptodon* than to the more heavily built *Diprotodon* or *Palorchestes*, and, also, that the "large tapir-like marsupial" of Papua is more likely, owing to its trunk-like snout, to be a New Guinean *Palorchestes* than a likewise immigrant *Diprotodon*. Both of these very judicious suggestions should replace my own in the checklist, and Janis should be congratulated for this improvement.

Concerning Victor Albert's Comment, I must admit that I deserve his severe criticism. He is absolutely right in stating that, by retracting my old hypothesis that a surviving thylacoleonid might account for the case of the Queensland Tiger, I yielded to the "lumper" approach I have always condemned myself in cryptozoological research. This was, of course, wrong, but I have some excuses for having done it.

It was certainly not in reaction to the convincing thylacine photos published in 1986, since I had already expressed this change of opinion four years before in the new introduction to the reprint of the original French edition of *On the Track of Unknown Animals* (1982, *Sur la Piste des Bêtes Ignorées*, Paris, François Beauval; Geneva, Famot). What actually happened was that, during the previous decade, numerous reports had accumulated on rather large, striped predators, sighted more often in the western and southeastern parts of Australia than in Queensland (that is, in the northeast), but described as more dog-like than cat-like.

On the other hand, because of information I received from Australia, I had become very suspicious about the authenticity of Ian Idriess' very interesting report on the more feline-looking carnivorous marsupial. Up to his untimely death, the Australian author always became very evasive when asked in private conversation to elaborate on his personal experience with the famous "tiger," and on the identity of the periodical where his report was originally printed. Moreover, an almost literal version of the latter, but attributed to somebody else, can be found, strangely enough, in D. H. Lawrence's celebrated novel *Kangaroo*. Who copied whom? And was the story at the start not just a literary fabrication? These embarrassing questions had to be asked.

All this resulted in my opinion gradually shifting from the fossil *Thylacoleo* hypothesis to the more plausible subfossil mainland thylacine hypothesis. It may well be, however, that *both* hypotheses are correct, and that two

large, striped, marsupial predators still live on the vast Australian continent, one being the mainland thylacine, whose survival cannot seriously be denied any more, and the other being a thylacoleonid dwelling in rain forest refugia, as Albert suggests. I would undoubtedly have accepted his conclusions had I known of his research when I prepared the checklist two years ago, or if I had been able to devote more time to updating and more carefully studying the dossier on Australian "tigers." But, as my checklist clearly shows, there are more than a hundred such dossiers.

Albert encourages, very opportunely, all cryptozoologists to specialize. In former times, I was forced myself to specialize in nonspecialization in cryptozoology, because somebody, at least, had to try and synthesize all the information which was then available on unknown animals, and to build up a solid and detailed method of research. This represented a tremendous amount of work. That is why—and how—I have been condemned, in a way, for more than 30 years for becoming a full-time professional cryptozoologist, and, unfortunately, I was the only one in the world. It is even possible that this is still the situation, as all the scientific researchers I know of indulge only sporadically in cryptozoological work, sometimes as a part-time job, sometimes as a leisure-time activity. Now that the way has been cleared, is not a race of professional cryptozoologists going to rise at last?

I would certainly encourage interested zoologists and anthropologists—particularly when they are as competent as Christine Janis, Victor Albert, and a few others I know—to specialize in cryptozoology.

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#### WHO IS AFRAID OF "HAIRY HORRIDS"?

(Comment on Groves, *Cryptozoology*, Vol. 5: 47–54)

I am thankful to Colin Groves for enlightening us non-Australians as to the exact ratio of marsupials and placentals on his island continent, and for stating his point unequivocally: "If there is a genuinely cryptozoological basis for any of the wild man (Yowie) stories, a wombat is quite certainly what it is" (p. 53).

However, I am very surprised and disappointed by the way he arrives at this conclusion. On the one hand, he concurs with me, saying that "other

reports certainly do not support the hypothesis that 'the' wild man in Australia 'is' a wombat, even the giant megafaunal *Phascolonius*" (p. 50). On the other hand, these perfectly realistic, down-to-earth reports are dismissed by Groves out of hand as "a hotchpotch of shooters' campfire tales, unidentifiable apparitions seen at dusk, and various hairy horrors that frightened the horses and demoralized the dogs" (p. 50).

He also takes me to task for "combining [these reports] together to create a hairy wild man" (p. 52). I am afraid this gives me too much credit. I did nothing more than humbly use the cryptozoological method in an attempt to arrive at the truth. By stating "one simply cannot do this," Groves denies a primary technique of cryptozoology. "Unidentifiable apparitions" they may be, but these comments and details supplied by eyewitnesses do help identification after all: "heavily built," "a big man covered with long hair," "like a black man, but covered all over with grey hair," "the face . . . like that of an ape or man, minus forehead and chin, with a great trunk all one size from shoulders to hips," "a man of powerful build . . . hair all over his body," "a gorilla-like man. He has long spindly legs. He has a big chest, long arms. His forehead goes back from his eyebrows. His head goes into his shoulders, no neck."

Similar eyewitness evidence, much of it coming decades later, has proceeded from the Himalayas, Siberia, North America, as well as the fossil record (in the latter case minus hairiness, of course). I am sorry Groves fails to face these stark anthropological facts. The ball is still in the Australians' court.

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#### THE YAHOO: A POSSIBLE SOLUTION

(Comment on Groves, *Cryptozoology*, Vol. 5: 47–54)

It is rare for an Australian zoologist to comment on such a peripheral issue as the reported Australian wild man. More importantly, a great deal of what Colin Groves states is very sensible and helpful, particularly his suggestion concerning the range of zoological options available. He has also drawn attention to some interesting new material. Nevertheless, I have doubts about the correctness of his fundamental approach to the subject, and about his understanding on some key issues.



At one point in his paper, Groves surveys reports about the Yahoo and asks the crucial question "What to make of them?" However, we are not offered the sort of critical analysis which might provide an answer because Groves has already made up his mind that what he refers to as the supposed evidence is just a disparate collection of tall stories. It becomes apparent that what he really means by that question is: how can one most easily explain these reports away? He does so by making some quite unsuitable comparisons, which enable him to conclude that the concept of the Yahoo has arisen through wrong identification, misunderstanding, or mental confusion on the part of the observer.

Groves has here and elsewhere (1984, *But How Many Large Terrestrial Animal Species Remain to be Discovered?*, *Cryptozoology*, Vol. 3: 111–15; 1985, *The Case of the Pygmy Gorilla: A Cautionary Tale for Cryptozoology*, *Cryptozoology*, Vol. 4: 37–44) made much of the idea that alternative explanations involving known animals can account for evidence supporting the case for unknown or poorly known animals. The proper technique, he suggests, is to look into the background of such reports, and analyze what is found before undertaking proper research. By applying those admirable principles, he has been able to show that there is really no basis for the nineteenth century notion of a pygmy gorilla.

In his new paper, under different circumstances, Groves has been compelled—in order to present known alternatives—to modify this process rather drastically, and he has done this by dispensing almost entirely with analysis, and proceeding at once to his conclusions. With the Braidwood animal, Groves notes only that body length was that of a very large wombat, and he concludes that this is what it probably was. The Bombala incident is inexplicably put down to a large kangaroo. The whole of the rest of the evidence for an unknown animal is then bundled together and dismissed with the gratuitous remark that people often see what they want to see. My point is that reduction of this sort has its place, but where applied, it must be shown to work.

The other aspect of Groves' approach about which I have reservations is his use, or rather misuse, of analogy. There is a double oversight here. First of all, the actual comparisons used are neither well made nor particularly apt. The introduction of the New Zealand moa is (to add to the zoological confusion) simply a red herring: Groves neglects to show that there is any real resemblance, let alone a "most striking" one, to the case of the Yahoo. Indeed, he himself has demonstrated well enough to require no further comment that the two were really quite different. The use made of the tiger stories is equally unsatisfactory, but one soon becomes inured to the severity of Groves' oversimplification. A second and more important error in method lies in the absence of any need to resort to analogy because the matter can be addressed directly, and assessed according to its own merits. In other

words, such diversions have the effect of again drawing our attention from Groves' failure to look closely at the matter in hand.

I suspect that Groves is inclined to give serious thought to a problem only when it admits of a solution drawn in terms of known zoological realities. Offending evidence can be interpreted as the occasion requires, or dismissed with nothing more than a perfunctory stab at justifying the means used to do so. Here perhaps is his "genuine science of cryptozoology": a sort of metamorphosis of the unfamiliar into the known. Groves is naturally much taken with Heuvelmans' treatment of the Nandi Bear because the matter can easily be given a perfectly simple explanation. Such a process accords with Groves' own experience in the field, which tells him that unusual occurrences almost always have a commonplace solution (Groves, 1984, above: 114). Considerations of logic and the evidence aside, the ultimate objection to all this is that it emphasizes preconceived ideas and discourages speculative thought. There is simply no room in such a system for anything new or uncertain. Therefore, we can learn nothing.

To be fair, Groves does look at another possibility, that of the survival of one or another of the giant marsupials, but it is isolated, as an afterthought, in his last paragraph. Perhaps it would have been worthwhile exploring that possibility a little further.

Groves is sometimes astray in his judgments on nomenclature. The modern meaning of *yahoo* is not an exact return to that of Swift (who conceived his *yahoos* as bestial sub-humans), but reflects a later usage derived from his. Again, it depends on what one makes of the infamous Yowie, but it is, at best, an oversimplification to suggest that *yowie* is merely a better known name today for the Yahoo. Furthermore, Groves suggests that *yahoo* may, after all, have been an aboriginal word, but the evidence he refers to concerns the bird of that name, and it is too slight to rule out the possibility that the word had been borrowed. I might add that the whole question of Aboriginal attitudes towards the Yahoo in the Brindabella region becomes a little clearer when we take into account a letter by George Graham Webb to the *Queanbeyan Observer* of July 31, 1903 (of which I was not aware until recently). In it, Webb ridicules and offers evidence against McKeahnie's opinions. It is also made clear that the Harry Williams referred to in Webb's later letter was an Aborigine.

I am glad that Groves brings up the subject of myth because, even though, as he points out, it is not a significant part of the evidence about the Yahoo, it is certainly an extraordinarily complex area. I cannot agree with him that all accounts based on Aboriginal reports are of a mythological nature. It is my impression that the later accounts especially do not show the characteristics of myth (Groves correctly notes that, in one case, linguistic evidence appears to support this view). Another difficulty which I have concerns the notion that a myth might be expressed using a borrowed name. Finally, it

may be worth reflecting upon the suggestion that, to a large extent, the subject of a myth (or each component thereof) is necessarily real, otherwise the internal characteristics which the myth conveys would not be capable of expression.

Further material on the Yahoo will be found in certain letters written during the early 1880's by the naturalist Henry McCooey. Although I have assembled these (Graham C. Joyner, 1986, H. J. McCooey, The Australian Museum and the "Indigenous Ape," MS., Australian Museum Library), the collection unfortunately remains unpublished. McCooey was never explicit about what he intended by the expressions "indigenous ape" or "Australian ape," but it is clear enough from his writings that he had in mind some marsupial counterpart to the large apes of Africa or Asia. In this connection, I have referred to a recent suggestion that a large marsupial, ecologically equivalent to the mountain gorilla in Africa, the giant panda in China, or the spectacled bear in South America, formerly inhabited montane rain forest in Papua New Guinea, and that *Hulitherium tomasetti* may have been capable of an upright, bear-like or panda-like posture (T. F. Flannery and M. Plane, 1986, A New Late Pleistocene Diprotodontid [Marsupialia] from Puren, Southern Highlands Province, Papua New Guinea, *BMR Journal of Australian Geology and Geophysics*, Vol. 10[1]: 65-76).

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#### NOISE, SIGNAL, AND EVIDENCE

(Response to Bayanov and Joyner)

In assessing cryptozoological reports, one should try to look through the "noise" and see if there is any "signal" behind it. By "signal," I mean consistency. As Bayanov notes, several reports of the Australian "wild man" mention that it is covered with hair (but not all reports even mention hair)—beyond that, we have description of the hair as grey in one report, long in another, but nowhere is it stated to be both long and grey. One report says the creature has a "big chest," but another describes a "great trunk all one size from shoulders to hips," which is not the same thing at all. What we need is two or more reports, independent of one another (and clearly not referring to a known animal), describing the *same series* of features. Then, and only then, can we claim to discern a "signal" amid the "noise." And

let us, too, admit that practical jokers exist, and try to remove their baleful influence on the matter!

I admit freely to Joyner that I have tried to "explain away" the reports. It is important to try to do so; if they cannot be explained away, then what we are faced with is Evidence; and, as I suggested above, the best real evidence is consistency.

Joyner asks a valid question: can the subject of a myth be real? I think that, sometimes, yes, myths have a real basis; equally, though, creatures can be dreamed up, as by a combination of the traits of various real animals. The archetype of such a mythical creature would, of course, be the Chimaera of Greek mythology.

As Joyner implies, the records are still coming in. Both he and I, in fact, have further data to publish, and Australian "hairy man" stories await full documentation. The interim record, however, indicates to me that there is no substance to such a supposed beast.

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#### PROPERTIES OF COLLAGEN AND THE NATURE OF THE FLORIDA MONSTER

(Comment on Mackal, *Cryptozoology*, Vol. 5: 55-62)

Roy Mackal's amino acid analysis of samples of the Florida Monster of 1896 is very interesting, but the results are to be taken with caution. It is quite clear that the tissue samples of the Florida Monster are almost pure collagen. Various works on this protein (Aurelio Bairati, 1972, Collagen: An Analysis of Phylogenetic Aspects, *Bolletino di Zoologia*, Vol. 39[2]: 205-248) have demonstrated that its molecular structure is based on a tripeptic sequence: -Gly-X-Y-Gly-X-Y- and so on, where X and Y are 2 amino acids, X being often the proline. As there is at least one glycine per 3 amino acids, glycine accounts for at least 33%—compared with the 34.6% of sample 2M.

That the tissue is mainly collagen is not a complete surprise: in 1897, Verrill himself finally thought that the organic material was connective tissue rich in collagen (Addison E. Verrill, 1897, The Florida Sea-Monster, *Science*, Vol. 5[116]: 476). The blubber hypothesis can no longer be supported after



Mackal's analysis, and, in addition, as blubber is only a thick layer of fat tissue, it could not account for the shape of the Florida Monster.

We know by radio-crystallography (the study of X-ray diffraction) that collagen has a very peculiar molecular structure, which recalls very much that of crystals, and one may commonly refer to the "paracrystalline state" of collagen. The cohesion of the molecular ordering is still reinforced by many H-liaisons ("hydrogen bridges"). Such a structure, which accounts for the cohesion and the mechanical resistance of this protein, could be expected for a protein found in tissues which must undergo hard mechanical strengths (bone, muscle, connective tissue). It also explains why the Florida Monster was so hard to cut open.

As for its chemical properties, collagen has a poor reactivity: almost all the proteolytic enzymes (enzymes which break the protein into its amino acids) have no effect on it, in particular the putrefaction enzymes (K. H. Gustavson, 1956, *The Chemistry and Reactivity of Collagen*, New York). This accounts for the fact that the Florida Monster remained unchanged for several weeks on a beach (at least from November 30, 1896, when it was discovered by two cyclists, till March 17, 1897, the date of DeWitt Webb's last known letter to William H. Dall about the stranded carcass).

But does all this prove that the Florida Monster was an octopus? Not necessarily. I agree with Mackal that, for biomechanical reasons, a large mass of collagen should be expected for a giant octopus, but there is another possibility, suggested by Verrill (1897, above) when he finally came to the conclusion that the Florida Monster was merely a part of a sperm whale's head, the spermaceti tank. This organ has a bag-like shape, it weighs several tons, and, above all, it is very rich in collagen; it could thus account for several characteristics of the Florida Monster.

The main problem is that the molar composition of collagen does not vary much from species to species. According to some biochemists, "there is a marked step up in the total of amino acids from poikilo- to homeothermic species with a complementary decrease in serine and threonine" (J. Pikkarainen and E. Kulonen, 1969, *Comparative Chemistry of Collagen*, *Nature*, Vol. 223[5208]: 839-41). Though serine and threonine are rather low in 2M, Mackal emphasizes that conclusions made from serine and threonine must be taken with caution, due to long storage in formaldehyde and ethanol, whereas glutamic and aspartic acids are not significantly different from the other samples.

However, I am more convinced than ever, for other reasons, that the Florida Monster was indeed a giant octopus, possibly a cirrate octopod, and I hope that immunological analyses will ultimately provide the solution.

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# WAS IT REALLY A YETI?

(Comment on Wooldridge, *Cryptozoology*, Vol. 5: 63-76)

The first Yeti photograph is indeed a memorable event, but was it indeed a Yeti? I have no quarrel with Anthony Wooldridge's data, his reasoning that the object viewed was not a langur, a bear, or a human, and I respect his interpretation that the object viewed and photographed was a Yeti. However, I offer the suggestion that the object was a rock protruding through the snow.

Several such rocks (seen in Wooldridge's Figs. 5 & 6) mark the two ridges bounding the steep, narrow valley across which Wooldridge was observing and photographing; those on the opposite (left) side are extremely close to the supposed figure of the Yeti. If one imagines the figure to have been a Yeti, it is naturally seen as erect, bipedal, and standing in snow. If, however, one imagines it as a rock, one sees, instead, a small, elongate mass, quite as the rocks close behind it (Fig. 6). The digitally processed image (Fig. 8) can easily be interpreted as a rock, identical in type to the rocks at the top of the same picture.

The artist's impression (Fig. 7) is, of course, just that, an impression by an artist who supposed that a Yeti was to be drawn. I myself, for instance, can interpret the rocks at the top of Fig. 5 as a series of objects, such as—starting at the top and moving down and to the right—a large parrot, the dragon of the Ishtar Gate at Babylon, a St. Bernard dog (the dragon and dog are superimposed and facing in opposite directions; the head of the dragon is the tail of the dog), a human with upraised arms, a small-headed human on all fours, and one of John Carter's thoats from Barsoom. As with isolated or numerous figures in clouds, different people can see different things.

Two other factors lead me to suggest that the "Yeti" is a rock protruding through the snow: 1) The picture is taken above the timberline; the shrub growing at the supposed Yeti's feet is at the limits of plant life for that altitude, and the rock (not the Yeti) has provided that iota of protection which has allowed the shrub to survive. 2) During an hour's observation, the supposed Yeti stood motionless in the snow, legs wide apart, with its feet getting colder by the minute. Obviously, if Yetis exist in that environment, they have adaptations for keeping their feet warmer than do most other animals; polar bears have thick corneal pads and hair on their feet, and the Alakaluf Indians of southern Chile wade unperturbed in frigid seawater, gathering shellfish (Carleton S. Coon, 1961, *Man Against the Cold: A Visit to the Last Alakaluf Shows their Adaptation to the Subpolar Climate*, *Natural History*, Vol. 70[1]: 56-96).

Obviously, we know little of the behavior or the anti-cold adaptations of supposed Yetis, but why would a hominoid stand so quietly in snow for an hour that an interested observer could not be certain that it had moved? One would expect that a large animal, in an environment where food was

scarce, would utilize the daylight hours to continue with its foraging. As to the shrub vibrating slightly, perhaps such movement was caused by a breeze, although none is mentioned by Wooldridge.

One part of Wooldridge's description, however, tips the balance toward the object having been a Yeti, and not a rock. Something left its sitzmark on the snow of the stalled avalanche, then stood up, and walked (presumably bipedally, but we are not told) to the spot by the shrub, where it was photographed (Fig. 5). If those tracks were indeed bipedal, and if they did proceed to the shrub, then that stationary figure would seem to have to have been a Yeti, and not a rock.

Whether the Yeti activated the avalanche or not is unimportant; there was an avalanche, and there was a slide mark on top of it, presumably of a size to have been made by a Yeti-size animal. The Yeti would have to have slid down on the snow of the avalanche after it had come to rest; to have come down with a moving avalanche would have risked being buried in it.

There was one way that Wooldridge could have solved the problem: Yeti or rock? He had the equipment to stay overnight in the open; alternatively, he could have descended to Govind Ghat or Joshimath for the night. If he had done so, and then returned the next day to the exact spot from where he had taken the photograph (Fig. 5), the Yeti, if that is what it was, would have been gone, but the rock, if that is what it was, would have remained unmoved.

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#### THE GARHWAL YETI: ALTERNATIVE EXPLANATIONS

(Comment on Wooldridge, *Cryptozoology*, Vol. 5: 63–76)

As an individual who has himself encountered "Yeti" evidence while conducting Himalayan fieldwork, I can well appreciate the personal ecstasy experienced by Anthony Wooldridge upon his most enigmatic sighting. As one who has carefully read the material written by Wooldridge, and who has corresponded with him requesting further information and photographs, I am convinced that he is reporting exactly what he experienced, and he is not in any way interpreting the data to fit the personal ecstasy of the event.

It is hard not to present such facts so they substantiate one's hypothesis. Wooldridge is to be congratulated in this regard, and in his willingness to communicate openly on his encounter.

Much of my life has been spent in the Garhwal Himalayas, where Wooldridge experienced his encounter. Unlike other areas further north and east in the Himalayas, where Yeti stories are rife, in this region the knowledgeable local hunters make no claims concerning the existence of the Yeti as an animal. Some local people, with only a layman's knowledge of their local fauna and flora, do have Yeti stories. In fact, a number of mythological animals are reported from this region; however, in detailed interviews I have conducted, a careful distinction is drawn between reality and mythology.

Following Wooldridge's report, I sent to precisely the same valleys that he visited a very reliable Tibetan fieldworker with whom I have worked for many years in the eastern Himalayas. This man, who personally believes in the Yeti, confirmed that, among local people knowledgeable on local fauna, he could find not a single claim of the Yeti as a living animal.

Consequently, starting from the very methodology of cryptozoology, Wooldridge's report is not in keeping with local beliefs. In fact, if such a sighting were ultimately demonstrated to be the result of a Yeti, it would contradict the validity of a fundamental cryptozoological premise: a discovery would have been made that is *contrary* to local peoples' knowledge. My experience has shown that great linguistic care and virtual dual linguistic fluency (in both the language of the villager and the Westerner) are necessary when using the cryptozoological method if a reliable context is to be built for local reports. Especially with regard to the Yeti, extreme precision and cross-checking is needed to separate reports of the mythical and the supposed real animal.

The photographs taken by Wooldridge look impressively like what we might expect the Yeti to look like. However, I would like to take this opportunity to make some cautionary remarks. Wooldridge's physical state at the time he took the photographs must be kept in mind. I myself have been in that same hypoxic (and possibly hypothermic) state often in the Himalayas, and then seen equally amazing sights. Wooldridge had rapidly ascended from the plains of India to a moderately high elevation of 12,500 feet (3,800 meters). He was not acclimatized. He necessarily must have been further hypoxic due to the truly amazing feat of running 30 kilometers that day, ascending at least 6,500 feet (more than 2,000 meters), much of it through very difficult terrain, with no road, and traveling through wet snow "above the tops of my training shoes."

Without doubt, Wooldridge was hypoxic from both exertion and altitude gain. Most probably, he was also mildly hypothermic from travel in wet snow. In the late 1960's, under similar circumstances in the Himalayas, I was convinced I had a bear in my sights. I took careful aim and fired three times. Carefully approaching my "kill," and only as I came within yards of it, did I realize it was a bear-shaped rock. Lack of oxygen and the cold had marred the clarity of my vision—but, surprisingly, not the accuracy of the tightly-grouped bullet marks on the rock!



A final cautionary point should be made before a Yeti explanation to these photographs is accepted. No known Himalayan animal ever stands stark still for 45 minutes. Furthermore, by all conjectures, Yetis must be extremely shy; hence, it is especially unlikely that a Yeti would stand still in the open, in an avalanche chute, for that long. The explanation that this may have been because the animal was dazed by a fall in the avalanche (and was presumably in some shock) is not in keeping with the known behavior of other mammals; once it is dazed, a mammal will attempt to lower its head below its heart-line to improve blood circulation, often rubbing its head as well. An undazed animal (certainly one with any intelligence, as the Yeti is conjectured to have) would surely have withdrawn from the avalanche chute as quickly as possible.

Wooldridge refers to the two sets of tracks he observed and photographed. The first set of tracks (not associated with the sighting), of which he kindly supplied me with a fine photograph, appears to me to be those of an ungulate, possibly a Himalayan goral. The second set (associated with the sighting), in my judgment, is not related to the potential Yeti at all. These second tracks, as I interpret them, pass about 2 feet behind the mystery entity, and continue on around the ridge.

In conclusion, alternative explanations for this sighting report are also worthy of consideration. The entity's shape on the photographic film could represent an upturned tree stump in the snow, or a rock outcrop sticking through the snow, which would not be considered remarkable by local people, but could well look like a Yeti to a person hypoxic and hypothermic. I suggest this with all kindness, as one who has been in that state repeatedly without realizing how it was affecting me. This tree stump or rock outcrop hypothesis cannot be proven, for when my fieldworker went to the area, and, with helpful directions and a map from Wooldridge, determined the location of the event to within a few hundred feet, such a tree stump or rock outcrop was not to be found.

The real question now is: did it walk away or roll away? Wooldridge is to be commended for providing us with a genuine cryptozoological mystery.

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## THE YETI: A ROCK AFTER ALL?

(Response to Reed and Taylor-Ide)

In May, 1987, I returned to the scene of my "Yeti" encounter of March, 1986, in the Garhwal Himalayas. The spindly bush was still there, but there was no visible evidence of any upright rock or tree-stump that might have misled me in 1986. Fig. 1 shows the bush with one companion standing just behind it, and another sitting on the snow to the right. The other objects are markers set up for a photogrammetry exercise. This photo was taken on May 16. Fig. 2 shows the bush in isolation; this photo was taken nine days later, on May 25, after some melting of the snow had occurred. Although the snow was still deeper than it had been in March, 1986, it seemed, at the time, that whatever I had seen and photographed by the bush in 1986 had now gone.

However, the results of the photogrammetric analysis, which I have just received (in late 1987), shed new light on the matter. Stereo pairs of photos taken in 1987 have been used to produce a three-dimensional map of the terrain near the bush. When this is used to derive an absolute scale for pairs



FIG. 1.—Photo of bush at the site of the supposed Yeti encounter of March, 1986, this new photo taken on May 16, 1987, during a return visit by the author. There is no visible evidence of an upright rock or tree stump. The author's associates are setting up markers for photogrammetry analyses.



FIG. 2.—Telephoto view of the bush taken nine days later, May 25, after some melting of snow had occurred. The snow was still deeper than when the March, 1986, photographs were taken.

of photos from 1986, it shows that, whatever I photographed in 1986, lies *below* the snow level in the 1987 photos. The object is leaning slightly uphill, and *no* movement can be detected when comparing photos taken at different times in 1986. The apparent change in position relative to the bush in some photos taken from different camera positions is caused by parallax. This evidence demonstrates beyond a reasonable doubt that, what I had believed to be a stationary, living creature was, in reality, a rock.

Further support for this negative conclusion comes from a photograph of the slope which I have recently received, which was taken, at my request, in late summer, 1987, by a local inhabitant. Although it was taken at a distance, and the terrain is mostly covered with vegetation, with no snow, there *is* a conspicuous rock feature on the slope by the bush. Unfortunately, the photo is of insufficient quality for reproduction here.

These two new pieces of evidence make me concur with the opinions of Reed and Taylor-Ide that, in 1986, I mistook a rock for a living creature. This conclusion, however, leaves unexplained what made the conspicuous slide in the avalanche debris, as well as the tracks leading away from the end of the slide. The tracks seen lower down (below the tree-line), and leading in the general direction of the avalanche, also remain unexplained; they are much too large to have been made by a goral, as suggested by Taylor-Ide.

I certainly sympathize with Reed's comment that I might have solved the problem by returning the following day in 1986; it would have saved me (and Taylor-Ide's fieldworker) some trouble in 1987. However, I had a tight schedule on my sponsored 1986 run, and if the snow had continued to fall heavily, it might well have covered the slope—and the rock in question—and confused the issue, as it did in 1987. Incidentally, I understand that Taylor-Ide's fieldworker visited the spot very soon after I had left in May, 1987, and, presumably, the snow still concealed the terrain—and the rock—near the bush at that time.

Naturally, I am very disappointed by the implications of the photogrammetry results. I had hoped they would support my original opinion that I had seen and photographed a Yeti. Nevertheless, I am very grateful to the painstaking technical work carried out by David Stevens of BKS Surveys and E. Wickens of University College, London. I am also grateful for the interest shown by Reed and Taylor-Ide.

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#### TRACING MONSTERS

(Comment on Bauer, *Cryptozoology*, Vol 5: 95–96)

In reviewing Joseph Zarzynski's book *Champ: Beyond the Legend*, Henry Bauer states: "According to Zarzynski (p. 65), and Richard Greenwell (p. 132), the object in the [1977] Mansi photograph [from Lake Champlain] bears comparison with that in the [1934] Surgeon's (Wilson) photo from Loch Ness; to this reviewer, no similarity is evident. In particular, the width of the Mansi 'neck' is about  $\frac{1}{8}$  of the length of the whole visible object; if the latter is indeed between 4.8 and 17.2 meters, then the diameter of the neck is between 56 and 150 centimeters (22 and 78 inches). Even the lower bound is almost twice the largest reported estimate for Nessie. . . ."

The purpose of this Comment is to clarify my position on this matter. In consulting p. 132 of Zarzynski's book, I find that I am quoted as stating—during my presentation in the 1981 Champ Seminar held in Shelbourne, Vermont—that the objects in the Wilson and Mansi photographs are "practically identical in size." I do not recall having stated that on any occasion, particularly as no reliable size estimates for either object had been produced



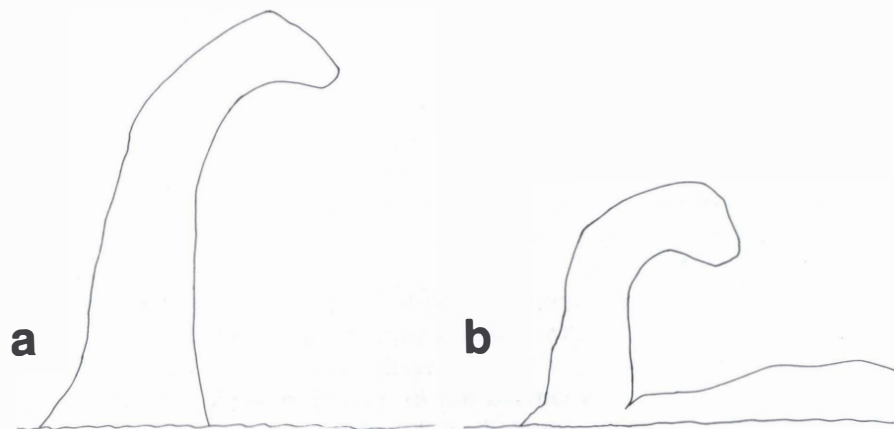


FIG. 1a.—Profile of the supposed animal appearing in the Wilson photograph taken at Loch Ness. Traced from slide projection.

FIG. 1b.—Profile of the supposed animal appearing in the Mansi photograph taken at Lake Champlain. Traced from slide projection.

by 1981. My talk at that meeting was informal, and I had few prepared notes; it is possible that I was misunderstood, and there is now no record of the event other than Mr. Zarzynski's book. The book also quotes me as stating that "the whole head and neck posture [in both photos] is very similar, [which could] lead one to speculate [it is] the same or similar species." I have no argument with this latter quote, as it does not convey any definitive conclusion, but I would prefer to replace "posture" with "configuration."

What I would now like to do, and what I did at the Champ Seminar, is demonstrate what I mean by the above. To do this, one may completely put aside the question of *absolute* sizes, and concentrate solely on *relative* sizes, such as the "neck" and "head" relationships. Fig. 1a is a tracing of the object in the Wilson photo. This was achieved by a simple method any reader can duplicate. One projects a slide of the photo onto a flat surface, such as a wall, on which tracing paper has been taped. Any desired size of the object can be projected onto the tracing paper by moving the projector closer to, or further from, the wall. The object can then be traced by following the enlarged outline of the projected image. The same was done to obtain the tracing of the object in the Mansi photo, appearing as Fig. 1b.

Leaving the tracing of the first object on the wall, one may then change slides and project the second object directly onto it, and this second object can then be traced over the tracing of the first object. The projector, of course, has to be moved closer to, or further from, the wall, until the two images are at about the same *relative* sizes; it is then that the similarities between the two presumed animals become striking, particularly when one

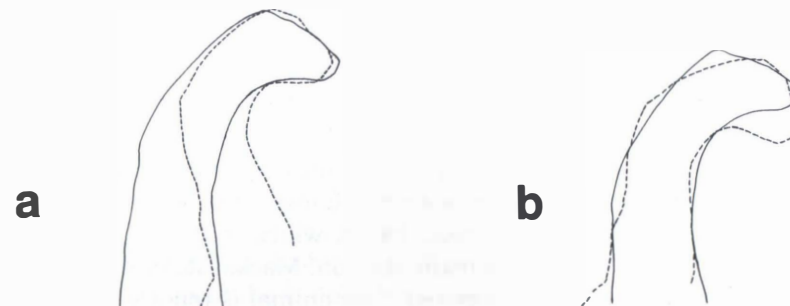


FIG. 2a.—Double tracing of the Wilson and Mansi profiles, with the "heads" aligned and the "necks" unaligned. Traced from slide projections. The Mansi profile is shown here in broken lines.

FIG. 2b.—Double tracing of the Wilson and Mansi profiles, with the "necks" aligned and the "heads" unaligned. Traced from slide projections. The Mansi profile is shown here in broken lines.

considers that the two photographs were taken thousands of miles and decades apart. The *absolute* sizes of the two presumed animals are of no concern to us whatsoever at this point.

Fig. 2a is a double tracing with the Wilson and Mansi "heads" aligned, and the "necks" unaligned. Fig. 2b is a double tracing of the "necks" aligned, and the "heads" unaligned. While the Wilson "animal" appears to be simply facing forward, and thus showing a relatively longer "neck," the Mansi "animal" appears to be turning back upon itself, giving a twisted, contorted look about it (and this is consistent with the Mansi's verbal account of the supposed animal; they described it as turning its head from side to side, as if looking about). Even so, it is quite evident by examining Figs. 2a and 2b that the *relative* "neck"/"head" proportions in the two supposed animals are very similar, and this is what I attempted to convey at the 1981 Vermont meeting. The disimilarities which *are* apparent could be better explained by differences in angle and posture than by taxonomic differences.

Since that time, new analyses have given us a better handle on *absolute* sizes. Paul LeBlond, at the University of British Columbia, has determined, based on the size of visible wind waves in the Mansi photo, that that object has a minimum length of 15 feet, 9 inches (4.8 meters), with an almost absurd maximum length stretching out to 56 feet (17.2 meters) (Paul H. LeBlond, 1982, *An Estimate of the Dimensions of the Lake Champlain Monster from the Length of Adjacent Wind Waves in the Mansi Photograph, Cryptozoology*, Vol. 1: 54–61). Having been close to the image processing work done on the Mansi photo at the University of Arizona, and having studied the photo extensively with the help of computer-generated images showing different contrasts and false colors, I have also made some calcu-

lations. Taking the more conservative, minimum LeBlond length of 15 feet, 9 inches (and one can speculate about the subsurface biomass if the object is indeed an animal), I have come up with a "neck" width of 23 inches—similar to Bauer's 150 centimeters or 22 inches—and a "neck"/"head" height of about 7 feet.

How does this compare with descriptions from Loch Ness? Rather than wade through the many books on the subject, I consulted Roy Mackal's *The Monsters of Loch Ness* (Swallow Press, 1976), which is the only work to have analyzed all the data in a systematic fashion. Mackal states (p. 89) that the height of the "neck" of the supposed Ness animal is reported to range up to 5 feet (about 2 feet shorter than the Mansi "neck" based on the minimum LeBlond length), with "neck" width  $\frac{1}{3}$  to  $\frac{1}{4}$  of that, or 12 to 20 inches (the latter maximum being 3 inches narrower than the Mansi "neck" based on the minimum LeBlond length). These size differences, then, are not that dramatic, and can certainly be explained by sexual dimorphism, age differences, or the occurrence of subspecies, or a combination of any of these. Why postulate two kinds of unknown animals with very similar morphology when we have enough trouble postulating just one?

More recently, LeBlond has studied the waves visible in the Wilson photo, and he presented his initial findings at the Society's July, 1987, Membership Meeting in Edinburgh. After further refinement, LeBlond (personal communication) has estimated the height of the "neck"/"head" of the Wilson "animal" above the waterline at 4 feet,<sup>1</sup> a little less than the 5-foot Ness maximum given by Mackal, and 3 feet less than the minimum "head"/"neck" height of the Mansi "animal."

While Bauer is correct in pointing out that the Mansi "animal" was considerably larger than the Wilson "animal," the relative "head"/"neck" proportions remain interestingly the same. Of course, besides the possible sex, age, and subspecific differences mentioned above, perhaps the model or models constructed for the possible hoax or hoaxes were simply of different sizes. However, if the objects photographed *were* animals—and I have never stated that I am convinced that they were—then I would venture to predict, perhaps in disagreement with Bauer, that they represent the same—or closely allied—species.

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<sup>1</sup> See Field Report by LeBlond and Collins elsewhere in this issue—Editor.

#### MORE ON CHINA'S WILDMAN

(Comment on Zhou, *Cryptozoology*, Vol. 5: 109–11)

Both Zhou Guoxing and myself feel that at least some claims for the existence of the Wildman are mistaken reports based on sightings of golden monkeys. Some disagree with this position (Jack Lapseritis, 1984, Golden Monkey versus Wildman: A Questionable Comparison, *Cryptozoology*, Vol. 3: 122–24). Golden monkeys and the reputed Wildman often live in the same area, however. In his 1984 Research Report, Zhou (1984, Morphological Analysis of the Jiulong Mountain "Manbear" (Wildman), Hand and Foot Specimens, *Cryptozoology*, Vol. 3: 58–70), suggested that hand and foot specimens supposedly belonging to a Wildman were actually from a macaque. Zhou now addresses this matter further, and he also discusses the taxonomy of Chinese macaques.

In January, 1987, I had the pleasure of visiting Professor Zhou at his laboratory at the Beijing National History Museum. We discussed the evidence for Wildman, and he kindly showed me a cast of the hand he had described and depicted in his 1984 Research Report. I fully concur with his judgment; the hand comes from a monkey, most likely a member of the genus *Macaca*, as Zhou had suggested. Zhou also possesses a number of hair samples and a plaster cast of a footprint supposedly belonging to a Wildman. Because of the poor quality of the latter, it will probably provide little useful information.

Reports touting the existence of Wildman continue to come in from China. The vast majority of such reports are unsubstantiated, are not made by scientists, and are old claims that have already been refuted in *Cryptozoology* and in a number of Mandarin-language publications that I am having translated.

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#### MORE ON "ETHNOKNOWN"

(Comment on Greenwell, *Cryptozoology*, Vol. 4: 1–14; and Debenat, Greenwell, *Cryptozoology*, Vol. 5: 113–16, 124–28)

If writers in general are happy with the term "ethnoknown," I can have no quarrel with that; part of the richness of language is that it cannot be prescribed. However, as my original comments on the term were quoted in



incomplete form, I think it worthwhile to set down, for the record, all the considerations which led me to my conclusions.

My objections to "ethnoknown" rest on three grounds: etymology, euphony, and sense. It is true that the elements of a word can be derived from different languages ("television" is the prime example), but this is exceptional, and such words usually meet the other two grounds for objection. Jean-Paul Debenat's counter-example of "brain therapy" is ill chosen: it is a two-word phrase, not a single word; and—leaving aside the fact that it does not mean the same thing—his suggested alternative "cervico-treatment" does not have Latin roots, but Latin and French roots. If a single word were required, I have no doubt that "encephalotherapy" (having Greek roots) would be used. The lack of euphony is largely subjective, and I shall not comment on it further.

On the grounds of sense, I realized (and wrote in my September 1, 1986, letter to Richard Greenwell) that the concept was broader than "known to the native inhabitants of a region." Nevertheless, that did seem to me to be the basis on which Greenwell was constructing the neologism. The sense of "known" may, *pace* Debenat, be adequately expressed by "-gnostic." Debenat's excursion into mysticism is largely irrelevant. The root is well established in English without any overtones of mysticism, as in the words "diagnostic" and "prognosis." *The Oxford English Dictionary* gives the primary meaning of the word "gnostic" as "relating to knowledge; cognitive; intellectual."

The reason that I wrote, as Debenat accurately quotes, "I'd settle for 'ethnognostic' if pressed," is that I see it very much as inadequate in itself. "Ethno-" does not adequately express the notion "native inhabitants." A native inhabitant is an autochthon, so the etymologically correct word to express the concept "known to the native inhabitants of a region" would be "autochthonognostic." This splendid word is at the same time accurate, almost unpronounceable, and monstrously sesquipedalian. It was the latter two factors which led me to concede the acceptability of "ethnognostic." "Ethno-" is very much a "buzz word" at present, and it is already misused and semantically stretched to such an extent that one more solecism won't matter. I did not write, as Debenat suggests, that "ethnoknown" is difficult to pronounce; my comment in the letter of September 1, 1986, that "ethnognostic . . . is more pronounceable," was in relation to "autochthonognostic," not "ethnoknown."

Greenwell's Response lays much greater stress on "prior human knowledge" than on "native knowledge." He compounds his etymological sins by writing that "Homo-known is another possibility, but that could lead to additional problems." Greenwell is confusing Greek and Latin *homo*: the root-forming element derived from Latin *homo*, "man," is *homino-*; the Greek root *homo-* means "same." The Greek root for "human" is *anthropo-*. If "prior knowledge" is to be the core concept (in which case the "hu-

man" element becomes otiose), then "proterognostic" fits the bill, *protero-* being the Greek root meaning "before."

I am flattered to learn that Bernard Heuvelmans had also expressed reservations about "ethnoknown," preferring "ethnognostic"; I look forward to possibly seeing "ethnognostique" occurring in his books. This brings me to a final objection to "ethnoknown." We live in an international scientific community, one of the advantages of which is that an international vocabulary is used with minimal adaptation in each of the major languages. "Ethnognostic" can become *ethnognostique* in French, *ethnognostisch* in German, and *etnognosticheskii* in Russian. In each case, we can recognize it as the equivalent to its English form. "Ethnoknown" cannot be adapted in the same way. A reading knowledge of English is essential for any serious scientist, including the readers of *Cryptozoology*; but that should not lead us into Anglocentrism. We should not forget that most of the key texts in cryptozoology were written in French.

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"ETHNOKNOWN" OR "ETHNOGNOSTIC"?

(Response to Heaney)

It is hard to force a new word into a pre-established mold. The new item may prove uneasy to grasp, evasive, and even slippery at times. However, if we invent a new term to fit a carefully designed mold, the final product is likely to arrive at an existence of its own when, and only when, it has been widely accepted. We cannot replace the users; they will accept the word or reject it.

The introduction of a new term has to serve a purpose. In the case of Richard Greenwell's "ethnoknown," I had the intuitive feeling that it actually filled a void. I suppose my intuition was nourished by articles and books on dozens of cryptozoological mysteries (for instance, the Ri, Mokele-Mbembe, or the wildmen of Kenya as described by Jacqueline Roumeguere-Eberhardt). Then, it appeared that, in most cases, the native inhabitants—or autochthons—knew of the existence of those elusive beings. Hence, the need for a term as concise as possible which might describe this "native knowledge." In that respect, I admire Michael Heaney's rigorous approach. Incidentally, his remark about the dubious etymology of the term "cervico-treatment"—which I provided as an alternative to brain therapy—is justified.

Let us tackle the core of the matter. In many an instance, the mystical and the rational overlap. This is especially true when we consider so-called primitive societies, or even the Tibetans whom Peter Matthiessen described in *The Snow Leopard* (Picador, London, 1980). While accompanying zoologist George Schaller during his search for the snow leopard, in 1973, Matthiessen undertook a long and exhausting personal quest. It is obvious throughout his book that his search for the snow leopard was secondary. Foremost in his mind was the quest for some ultimate form of knowledge. His curiosity was directed towards those who might provide him with some clues: monks and priests, guides and peasants, all of whom appear, to varying degrees, as people who "know"; in other words, as gnostics.

At one point, speaking of "the doctrine of Maya that emerges in the Vedic civilization and may well derive from much more ancient cultures," Matthiessen writes: "Maya is Time, the illusion of the ego, the stuff of individual existence, the dream that separates us from true perception of the whole."

For the early Gnostics, man was but a shadow who owned but a shadow of a conscience. In everyday life, they tended to reject human institutions, for they thought that the world was governed by simulacra of men. They would not abide by the reign of the "pseudanthropos." In his foreword to the study *Les Gnostiques* (Jacques Lacarrière, Gallimard, Paris, 1973), Lawrence Durrell underlined the nobility of the gnostics' poetic challenge. Such maybe qualified Matthiessen's modern quest, noble and brave too, in an attempt at perceiving "the whole so-called spirit-world," beyond the barriers of both the ego and men's institutions. The same notion was expressed by popular singer/poet Leonard Cohen in *The Guests*:

And there they take their sweet repast  
While house and ground dissolve  
And one by one the Guests are cast  
Beyond the garden walls.

What is to be found, indeed, beyond the walls? I would not want to take sides, as the Catholic philosopher and teacher Jean-Noel Dumont did in "La Gnose de San-Francisco," an article on San Francisco writer Philip K. Dick's religious speculations: "Behind the twin walls of gnostic architecture and irony, there is certainly nothing but the cold chamber of despair" (*Science Fiction*, No. 7-8, Denoel, Paris, 1986).

The aim of this *brief* incursion into Gnostic territory is not to display, in a pedantic way, the extent of my erudition—quite limited—in that field. I am mainly trying to focus the reader's attention on one point: the gnostic trend is still alive and far from disappearing. A number of studies on the subject may remain restricted to the academic community (such as *Gnosticism and Early Christianity* and *The Anthology of Gnosticism*, by Robert M. Grant); nevertheless, one cannot deny that Matthiessen's books, Leonard

Cohen's songs, or Philip K. Dick's novels (and, in particular, his "meta-physical" trilogy, i.e., *Valis*, *The Divine Invasion*, *The Transmigration of Timothy Archer*) have reached a wide audience. Since gnostic philosophy, or at least some form of gnostic speculation, does survive, why shouldn't we grant the word "gnostic" its full meaning as defined, precisely, by *The Oxford English Dictionary*?

I must admit, though, that the word "gnostic" is not often heard in everyday conversation. More common is the use of "agnostic," by which people describe their lack of religion. *The Oxford English Dictionary* defines this term as follows: "One who holds that nothing is known, or likely to be known, of the existence of a God or of anything beyond material phenomena." Here, the overtones of mysticism can hardly be ignored.

As I do not wish to embark on a lengthy discussion involving further linguistic finesse, I shall merely insist on one last point. In order to give full life to an event, a living creature, a philosophical movement, a social behavior, etc., we have to name it accurately. Otherwise, it would be as if the "thing" in question had not reached its plenitude. In a way, we have to help it express its own reality.

This is what is at stake in this controversy. The term "ethnoknown" sounds neutral: it does not exclude any form of knowledge. As to the term "ethnognostic," it implies, in my view, *something beyond material phenomena*.

In the end, I rest assured that my own personal opinions will carry little weight: the public will decide, and they may even refuse both terms.

But, in any case, let us be neither philoprogenitive nor, of course, sesquipedalian.

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#### THE RI IS THE IRRAWADDY DOLPHIN

(Comment on Williams, *Cryptozoology*, Vol. 4: 61-68; and Greenwell, *Cryptozoology*, Vol. 5: 140-44)

The mysterious creature called Ri (or Ilkai), supposedly an aquatic, mermaid-like animal of New Ireland, Papua New Guinea, has caused a lot of



speculation, and even two expeditions. However, the identification of the animal as the Indo-Pacific dugong (*Dugong dugon*) cannot be the last word on the subject. Both Thomas Williams and Richard Greenwell seemingly brush aside—or do not account for—many details commonly attributed to the Ri, and stressed as such in previous publications: the Ri's diet of fish, its habit of swimming upstream in freshwater rivers, and, most important, its peculiar body configuration, with a highly mobile human-like or monkey-like head on a distinct neck, which, apart from its vegetable diet and strictly marine habitat, the dugong does not sport.

Furthermore, the darkish, colossal dugong with its bull-dog face would certainly not evoke comparisons with a "light-colored," elegant water nymph. Eyewitness reports concurring on such points as are contrary to dugong appearance or behavior must therefore be speaking of a different animal. I am not convinced that the finding of casual dead dugong resolves the problem once and for all, as both Williams and Greenwell seem to be. I dispute the "overall" identification, as well as the dugong being the only possible explanation or candidate for explanation.

Roy Wagner himself originally stated: "Whatever the *ri* may be, they are certainly not dugongs" (Roy Wagner, 1982, "The Ri—Unidentified Aquatic Animals of New Ireland, Papua New Guinea," *Cryptozoology*, Vol. 1: 33–39). Consequently, Williams' identification may be based on his native informants' applying the same name(s) to two similar but zoologically distinct species, or, more likely, on a deliberate misidentification on the part of good-humored, native informants wishing to please expedition members. Villager Tom Omar, who identified a dugong swimming in Nokon Bay as an Ilkai, and who went on to describe the creature as having a woman's face, hands, and breasts, neither got a close look at the live animal nor knew what a dugong looks like. He gave the description of a mermaid (which he had seen on match boxes), which he thought the expedition was after. His description of an Ilkai, and that of other native informants referring to the Ri, did not match the appearance of a dugong. So, these cannot be called an "identification." Had the dead dugong the expedition eventually encountered been, instead, a cetacean, the villagers, I am sure, would have called *that* animal a Ri or Ilkai. Therefore, the "identification" should be reconsidered in light of other options.

The first step in an identification procedure should have been an attempt to interpret the name of the animal (to say nothing of tracing it etymologically). This would have resulted in the finding that the coastal Motu-speaking tribes in the Central District of Papua have a particularly long tradition of dugong hunting, their name for this sirenian being *rui*, which is quite similar to the New Ireland word *ri* (whereas the Pidgin English word for the dugong is *bulmakau bilong solwara* or *bonon*, and the central New Ireland Barok-language word is *bo narasi* ("pig of the ocean," not "nymph"!)). The New

Guinea distribution of the dugong is from the Eilanden River in West Irian, around the Papuan coast, and through the New Guinea islands; also around Manus Island, New Ireland, and Bougainville. This would have solved the mystery at once, and without the necessity of an expedition: *ri* = *rui* (with the latter known to be the dugong). Consequently, such New Ireland sightings and encounters as are connected to the word *ri* may actually be based on dugongs.

However, although it became apparent that, further north of central New Ireland the dugong and the *ri* are regarded as the same animal, it was found that, around Ramat Bay and further south, "the distinction becomes clearer, and two separate animals are described" (Wagner *et al.*, 1983, Further Investigations Into the Biological and Cultural Affinities of the Ri, *Cryptozoology*, Vol. 2: 113–25). Therefore, the *ri* proper would seem to be an animal somewhat *similar* to the dugong, but nevertheless different. The correct native word would seem to be the Susurunga *ilkai*, as encountered by the Wagner team some 50 miles south of Ramat Bay (Wagner *et al.*, 1983, above).

The second step in deducing the identity of the animal should have been to identify it from among the known marine fauna of Papua New Guinea. This sort of literature research was indeed conducted by Wagner *et al.* (1983, above). They correctly ruled out known sirenians (other than the dugong), as well as pinnipeds, and came to the conclusion that "the cetaceans are the only other candidates" (p. 124). It is the more surprising that neither the combined efforts of Wagner and his three co-workers nor the personal communications of the two specialists they consulted led to the selecting of a suitable candidate from the order of cetaceans. As very little has been published on the cetaceans of Papua and New Guinea waters, the dozen species recorded to date should have been properly reviewed. For further basic literature obviously not consulted, I refer readers to W. H. Dawbin's article "Dolphins and Whales" in the *Encyclopedia of Papua and New Guinea* (1972, Melbourne University Press, Vol. 1: 270–76).

There is, indeed, a cetacean species which *does* fulfill the requirements, and which seems to have been overlooked by Wagner *et al.* (1983, above) and their consultants, as well as later by both Williams and Greenwell. The Irrawaddy dolphin (*Orcaella brevirostris*), originally described from the Irrawaddy River of Burma, was, until recently, thought to inhabit larger rivers and coastal waters between the Bay of Bengal and Borneo. Dawbin (1972, above: 274) quoted data published elsewhere which recorded sightings of this "river" dolphin throughout Indonesian coastal waters to the north of West Irian (the Indonesian part of New Guinea), as well as specimens accidentally caught in fishing nets in the Gulf of Papua, and others photographed off Australia's Queensland coast to the south of Cairns. Dawbin stated that the species had so far not been recorded off the north coast of



FIG. 1.—The Irrawaddy dolphin, *Orcaella brevirostris*.

New Guinea, nor in waters further east. The *ri/ilikai* sightings will close this gap.

The Irrawaddy dolphin (Fig. 1), unlike any other dolphin in the region, does indeed look strangely un-cetacean, and, if anything, sirenian or “mermaid-like,” as it has a rounded and completely beakless head, and a distinct neck said to be surprisingly mobile for a cetacean. The flippers are blunt, like “arm stumps,” and the dorsal fin is very small and not visible whenever the fore part of the animal shows above the surface (the fin is situated rather back along the body). The color of the Irrawaddy dolphin in the waters of New Guinea is said to be light brown above, merging to cream or dark cream below. The animal swims relatively slowly, and has been seen within a mile or so off shore. It travels in small groups of six individuals or less, and it is exclusively a fish eater. The animal is capable of much more twisting and flexing of the body than most other dolphin species, and, according to Dawbin (1972, above: 274), “the combination of their size, color, blunt head, and overall appearance has led to this species being confused with dugongs.” Their size is roughly that of a human; they may reach nearly 2.05 meters (almost 7 feet) in length.

Thus, the native description given by Wagner *et al.* (1983, above: 115): “Human-like in the upper torso, with arms closed in or ‘fused’ to the sides of the body, smooth skin below (no scales), and light brown in color,” along with the animals’ “extreme vertical flexure” (p. 124), and the photographic evidence of an unsirenian tail fluke (p. 119), match the general appearance of the Irrawaddy dolphin, as do other supposedly legendary details of the Ri’s natural history: the “romantic” description of “a line of *ri*, male, female, and juvenile swimming up a freshwater stream by moonlight” (which strongly points to the habits of a river dolphin); the animals’ speaking “in whistles or whispers,” or uttering “an almost human cry” (details that suggest a cetacean’s whistles, palaver-like sounds and piercing shrieks); the animals’ “mammalian breasts” and “human-like genitals”; the color of the blubber, etc. (all quotations from Wagner, 1982, above).

Summarizing, it seems safe to conclude that the *ri/ilikai* constitutes pres-

ently unknown populations of the Irrawaddy dolphin on the north coast of New Guinea (see Wagner, 1982, above: 38, on *ri* off Aitape), around the central and southern shores of New Ireland, and “through the stretch of water that separates New Ireland from Buka and Bougainville Islands in the Northern Solomons” (Wagner, 1982, above: 38).

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#### IS THE RI THE IRRAWADDY DOLPHIN?

(Response to Sehm)

Gunter Sehm attempts to make the point that the animal known as *ri* or *ilkai*, which we identified during our 1985 expedition to New Ireland, Papua New Guinea, as the Indo-Pacific dugong (*Dugong dugon*) is not the dugong, but, rather, the Irrawaddy dolphin (*Orcaella brevirostris*).

While discounting the testimony given by natives linking the creature we observed (which unequivocally *was* a dugong) to the on-the-spot description of the Ilkai by Tom Omar (and corroborated by the other Nokon villagers), Sehm seems willing to accept native eyewitness descriptions of a mermaid-like creature as evidence that some animal answering those descriptions does exist. But certainly an Irrawaddy dolphin seen up-close looks no more like a mermaid (distinct neck, long hair, breasts, and arms) than does a dugong!

The sad fact is that the Susurunga villagers of Nokon have no real idea of what either of these animals looks like up-close. They are not dugong hunters, nor are they fishermen; as pointed out by Greenwell, they live by the sea, but appear to have little interaction with it—besides telling tall tales. In fact, the women of Nokon, when questioned about the Ilkai by the female members of our team—out of earshot of the menfolk—would smile condescendingly and say: “Well, yes, that’s what the men say.” We thus face the possibility of some kind of mutually reinforced male fantasy projected onto some convenient but not readily identifiable animal. If the Irrawaddy dolphin happened to be available in the area, it, too, might be singled out as the Ri or Ilkai. I consider this to be a phenomenon that would be interesting to explore on its own merits.

But where do Sehm’s arguments really take us other than back into the realm of armchair cryptozoology? First, there are some logical inconsistencies. If the (mermaid-like) animal known as the Ri and the dugong can



be considered the same animal on linguistic grounds, then why cannot an animal answering the Ri's description, but given a different name, also be regarded as the same? Sehm's claim that *ilkai* is probably the "proper native word" for the animal displays an ignorance of, and arrogance toward, what Westerners refer to as "natives."

In a nation such as Papua New Guinea, with over 600 languages, what is the "proper native word" for anything? *Rui* is indeed the Motu word for dugong. Motu is a language native to the (southern) Port Moresby coastal area. It was spread as a kind of *lingua franca*, or "police Motu," by native constabulary during British and Australian colonial rule. Nokon, in New Ireland (an island province), is more than a thousand miles away, over open ocean; furthermore, New Ireland was under German control until after World War I (and under Japanese control during World War II), so establishing a real linguistic link between *rui* and *ri* would be very tenuous at best.

I am not prepared to state that our own findings "close the book" on the question. Those words were Greenwell's, not mine. Briefly, however, our findings corroborate the observations of Wagner *et al.* (1983, Further Investigations Into the Biological and Cultural Affinities of the Ri, *Cryptozoology*, Vol 2: 113–25), including previously unreported dive durations and extreme vertical flexure. The animal under observation was described as the Ilkai, and it was subsequently observed more closely—and photographed—underwater by three divers *while* its surface behavior was being kept under constant observation by other expedition members. The animal found dead on the beach the very next day was the same species as that observed underwater and described as the Ilkai. It was the Indo-Pacific dugong, *Dugong dugon*.

I flatter myself in thinking that we conducted our expedition according to proper scientific methods. On the basis of others' observations, and our own research, we formed a hypothesis and a plan of action. We went to the area, made observations, gathered data, and returned with conclusions based on those observations. The next step in the scientific method—for those who think it is necessary—is for others to be able to repeat our results—or to modify our conclusions with a hypothesis of their own, which would, in turn, be subject to test.

I invite Sehm to rise from his comfortable armchair in Hamburg, and to go to Papua New Guinea and test his hypothesis against our results. I would be most intrigued to see if he could, in fact, establish a credible link to the Irrawaddy dolphin, and I would be more than delighted if he could present evidence of a truly unknown animal at the bottom of the Ri/Ilkai stories. Our results did not do that, and we cannot, on the basis of those results, pretend that they did.

But the real point of this discussion is one of method. Science is really the world's most democratic activity. Anyone can participate, provided they

play by the same rules, which are basically quite simple: observation, hypothesis formulation, testing, and producing results which are repeatable. Extrapolation and theorizing on the observations of others can only take one so far.

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#### IN DEFENSE OF BIG CATS

(Comment on Bottriell, *Cryptozoology*, Vol. 4: 80–83, Vol. 5: 147)

Concerning the British Big Cat problem, surely the hundreds of eyewitness reports collected by Di Francis deserve more consideration than given by Lena Bottriell. Her fieldwork on Exmoor and Dartmoor has revealed Big Cat sightings dating back at least 60 years. Bottriell does not actually state that the British wild cat, *Felis silvestris grampia*, inhabits Exmoor, but it would, in any case, hardly be capable of killing full-grown sheep.

A feral domestic/wild cat cross might be slightly larger than its pure-bred parents, but it would not be unreasonably longer, such as the reported 4 feet. The case of three "lynxes" found in Inverness-shire, Scotland, in 1927 is well documented, and, despite Bottriell's negative review, I still feel that there is a large, unknown cat in the wilder areas of Britain, perhaps even a form related to the early Pleistocene puma, and possibly even the snow leopard.

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# A LIVING DINOSAUR?

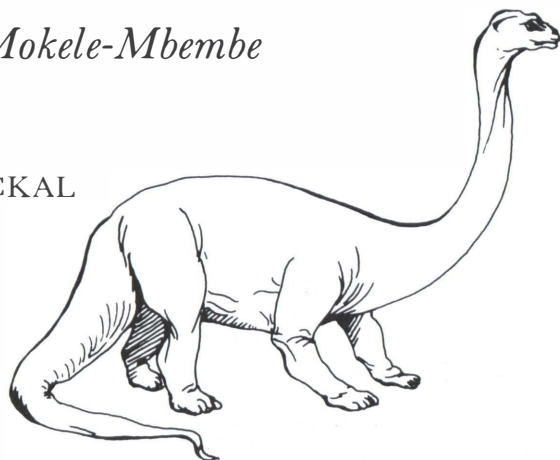
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All manuscripts submitted for publication in *Cryptozoology* must be typed double-spaced (if possible on 8½ × 11 inch or 22 cm × 28 cm sheets). American rather than British spelling and style are preferred (e.g., periods and commas placed *inside* of quotation marks). The author's last name must appear in the top, right-hand corner of every manuscript page.

### Articles

Manuscripts must be submitted in triplicate, and must be prefaced by 50–100 word informative abstracts. The abstract should appear on the bottom half of the title page. The manuscript should begin on the second page. The citation of references is not mandatory, but is preferred. If references are cited, the author(s) must conform to the referencing instructions appearing below. Manuscripts should contain relevant descriptions or interpretations related to cryptozoological matters. Topical or theoretical discussions or literature reviews, rather than specific research findings, are preferred. Illustrations and maps are welcome, but original artwork and black and white photographs are required (color slides are not acceptable). The author(s) should design subheadings in accordance with the structure of the article. Manuscript length is left to the discretion of the author(s), although a minimum of 1,000 words is expected. Manuscripts are reviewed by two referees for scientific content, originality, and clarity of expression, and the Editor may suggest deletions or revisions before acceptance.

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Notes and footnotes should *not* be included. Authors should attempt to include such comments within the text itself. Contributors are urged to consult an issue of the journal and/or consult the Editor when in doubt.

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